Hearing Record of the Public Hearing on the Draft Environmental Impact Statement

Livermore Site
Livermore, California

DOE/EIS-0028-D

Livermore, California
April 12, 1979

Held by

U.S. Department of Energy

Hearing Record of the

Public Hearing on the

Draft Environmental Impact Statement

Livermore Site
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April 12, 1979

Members of the Presiding Board:

John B. Farmakides, Chairman
Dr. Trowbridge Grose
Dr. G. Victor Beard

Held by:

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Index to the Hearing Record of the

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Tab 1

Federal Register Notices

Public Hearing -- 44FR11821, March 2, 1979

Availability of DEIS -- 43FR43379, September 25, 1978

[6450-01-M]

DEPARTMENT OF ENERGY

LIVERMORE SITE, LIVERMORE, CALIF.

Public Hearing on Draft Environmental Impact Statement (DOE/EIS-0028-D)

The Department of Energy issued the draft Environmental Impact Statement, Livermore Site (DOE/EIS-0028-D) in September 1978, for public review and comment, with a three-month comment period. The draft Environmental Impact Statement (DEIS) was prepared in compliance with the National Environmental Policy Act (NEPA) to assess the environmental impact of continued operation of the Lawrence Livermore and Sandia Laboratories located in Alameda County near Livermore, California. The EIS assesses the potential cumulative environmental impacts associated with the current site activities, including the operation of the laboratories, and with the alternatives to current operations at that site.

Notice is hereby given that DOE will conduct a public hearing in connection with the draft statement commencing at 9:00 a.m. on April 12, 1979, at the Granada High School Auditorium, 400 Wall Street, Livermore, California.

The purpose of the hearing is to afford further opportunity for public comment regarding the DEIS. In order to sharpen and focus the major issues for discussion and examination at the hearing, DOE will make available a staff statement summarizing and addressing the substantive areas of concern raised in the written comments received on the DEIS.

The areas of concern include: (1) Mission and location of the laboratories; (2) health effects and dose calculations; (3) seismology and hydrology; (4) emergency plans; (5) environmental monitoring analysis and standards; (6) accident analysis and central systems; (7) transport of radioactive materials in the environment; and (8) transportation of radioactive materials.

The above issues and other issues raised at the hearing will be addressed and appropriate revisions made in the EIS, which is expected to be issued later this year in final form.

The hearing will be conducted by a three-person Presiding Board selected by DOE. The Chairman of the Board and one other member of the Board will consititute a quorum. The Chairman of the Board will be Mr. John Farmakides, an administrative law judge who is Chairman of the Contract Appeals Board at DOE. The other two members of the Board are Dr. L. Trowbridge Grose, Professor of Geology at the Colorado School of Mines, and Dr. G. Victor Beard, Physical Chemist and Professor of Nuclear Engineering, at the University of Utah.

Persons, organizations, or governmental agencies wishing to appear and make a presentation are encouraged to become "full participants" in the proceedings by filing with Mr. W. H. Pennington, Deputy Director, Office of Environmental Compliance and Overview, U.S. Department of Energy, Mail Station E-201, Washington, D.C. 20545, (301) 353-3034, not later than 5 p.m., EST, on April 5, 1979, a notice of intent to participate. The notice shall set forth: (1) The name and address of the participant and his representative, if any; (2) the nature of the participant's interest in the proceeding; (3) the text of any statements to be presented at the hearing, or a reasonably detailed, summary thereof; (4) the names and addresses of all witnesses to be produced at the hearing by the participant and a summary of the substance of the proposed testimony; and (5) the amount of time desired to complete the presentation. The Presiding Board will endeavor to schedule the full amount of time requested by full participants (those who file a notice of intent to participate) subject to the imposition of such reasonable time limits as are consistent with orderly procedures and as will assure other full participants a meaningful opportunity to present their views.

Persons, organizations, or governmental agencies wishing to participate, but who do not file a notice by 5 p.m., EST, on April 5, 1979, may notify Mr. Pennington before the hearing or the Presiding Board during the hearing of their desire to make a presentation. Such parties shall be admitted as "limited participants" and shall be heard at such times as the Presiding Board shall permit for a period of not more than 15 minutes each, unless the Presiding Board, in its discretion, allows additional time.

The public hearing will be legislative rather than adjudicatory in nature. Discovery, subpoena of witnesses, cross-examination of participants, testimony under oath and similar formal procedures appropriate to a trial-type hearing will not be provided. Participants will reference and produce, on request of the Presiding Board, the documents on which they rely.

DOE will make available appropriate witnesses to explain the background, purpose and environmental impacts of the laboratories at Livermore and to respond to appropriate questions. Questions may be posed to participants (including DOE staff members) during the course of the hearing by other participants (including DOE staff members) and the Presiding Board, either orally or in writing, provided that: (a) All questioning shall be subject to the control and discretion of the Presiding Board, (b) questions shall be permitted from limited participants only to the extent that they are relevant to the issues identified in the staff statement, unless the Presiding Board determines that additional questions are necessary to develop an adequate record, and (c) any participant (including DOE staff members) may elect to answer any such questions either orally at the hearing or in a written submittal to be filed with the Presiding Board before the close of the hearing record, which date shall be determined by the Board.

A transcript of the hearing will be made. The Record of the hearing shall consist of the transcript, and all documents received into the record by the Presiding Board.

After the close of the hearing record, the Presiding Board shall render its Report. The Report shall be based upon the Presiding Board's review of the DEIS and the hearing record and shall: (a) Identify those unresolved issues raised at the hearing which the Presiding Board deems to be critical to future decisions concerning the operations and reasonable alternatives, and (b) present the recommendations of the Presiding Board concerning the treatment of these issues in the final environmental impact statement in a manner which will assure informed decisionmaking. In discharging its responsibilities, the Presiding Board shall not undertake to resolve issues or render judgment concerning the operations.

The Record and the Board Report will be fully considered and taken into account in the preparation of the final environmental impact statement and in making decisions. The Record and the Board Report will be made available for public inspection at the locations noted below as soon as practical after the close of the hearing. Copies of the DEIS, the formal comments received, and the staff statement are available for public inspection at the DOE public document rooms located at:

Public Reading Room, FOI, Room GA-152, Forrestal Building, 1000 Independence Ave., SW., Washington, D.C.

Albuquerque Operations Office, National Atomic Museum, Kirtland Air Force Base East, Albuquerque, New Mexico.

Chicago Operations Office, 9800 South Cass Avenue, Argonne, Illinois.

Chicago Operations Office, 175 West Jackson Boulevard, Chicago, Illinois.

Idaho Operations Office, 550 Second Street, Idaho Falls, Idaho.

Nevada Operations Office, 2753 South Highland Drive, Las Vegas, Nevada.

Oak Ridge Operations Office, Federal Building, Oak Ridge, Tennessee.

Richland Operations Office, Federal Building, Richland, Washington.

Energy Information Center, 215 Fremont Street, San Francisco, California.

Savannah River Operations Office, Savannah River Plant, Aiken, South Carolina.

In addition, the above documentation will also be available for public inspection at the Visitor's Center at the Lawrence Livermore Laboratory.

Copies of the staff statement can be obtained from Mr. Pennington, the Energy Information Center in San Francisco, or the Visitor's Center at the Lawrence Livermore Laboratory.

Dated at Washington, DC this 27th day of February 1979.

For the United States Department of Energy.

RUTH C. CLUSEN,
Assistant Secretary for Environment.

[FR Doc. 79-6238 Filed 3-1-79, 8:45 am]

Tab 2

Letter from Presiding Board to Mrs. Ruth Clusen (May 17, 1979) transmitting the Statement by the Presiding Board and Appendices--Exhibits and written Statements Submitted at the Hearing

Specific Comments

1. p. 2-18, section 2.1.6.5, last sentence: Please identify the "accepted standard."

2. p. 3-1 to 3-2: Will the run-off from the area drained by Arroyo Las Positas and the other areas feeding the man-made lake cause any significant accumulations of radionuclides in the lake?

3. p. 3-15: At what frequency are the pre-HEPA filters (glove box filters) changed in building 251? Are they disposed of as transuranic waste (i.e., above 10 nanocuries per gram)?

4. p. 3-17: There is a conspicuous absence of a building drain retention system for building 331. This building is the major source of gaseous tritium releases and it is understood that typical tritium liquid effluents originate from equipment contamination in building 419. The FEIS should clarify the relationship between decontamination of building 331 equipment and building 419. The liquid effluents from building 419 should also be addressed. In the future it is possible that a water fire-protection system would be installed in building 331. The FEIS should discuss briefly the mitigating measures that would accompany this decision.

5. p. 3-36 to 3-37: The radiological impact section on these pages is lacking much significant information. EPA believes that the additional information requested in the following items is necessary for a proper and thorough evaluation of the facility's radiological impacts:

a. With the exception of the few maximum individual dose results reported there is no mention of what radionuclides were considered in the assessment. It is necessary to know the significant impacts from the other radionuclides, along with the definition of what is considered significant, to provide a complete picture of the impact.

b. It is necessary to assess population doses as well as the individual doses discussed in "a" above. From this should come health effects estimates in the form of morbidities, mortalities, and genetic effects.

c. There needs to be a presentation of the assumptions and methods used in preparing the dose assessment. The methods, i.e., models, presentation could simply be a reference to the available literature, if appropriate.

d. From this presentation it appears that only direct and airborne pathways were considered. Food and water pathways must also be considered or adequate reasons given for not considering them. The data associated with them should also be presented to the extent that a reviewer could independently evaluate them.

e. There is currently proposed Federal Radiation Guidance from EPA concerning levels of transuranics in the general environment (EPA/4-77-018). While this Guidance has not yet been signed by the President, DOE should consider presenting a comparison of the expected doses to individuals from alpha radiation from transuranics released from LLL/SLL with the levels provided in the Guidance, viz., one millirad per year to the pulmonary lung or three millirad per year to the bone.

6. Section 3.9: The analysis of both historical and postulated accidents and effects was generally well done. The following items would further strengthen this portion of the EIS:

(a) Provide the X/Q values for specific locations such as the nearest residence, nearest cow and pasture, and nearest school;

(b) Provide estimates of individual doses, population doses, and health effects received from historical accidents, if available;

(c) Provide the age groups being assessed; and,

(d) Provide health effects estimates for postulated accidents.

7. p. 3-47, Section 3.9.1: Specify the "appropriate radiation or concentration guides.

<u>Comments not related to radiation</u>

1. p. 2-59: It is noted that DOE believes that Site 300 is the only known natural location for the wildflower known as Amsinckia grandiflora. The DEIS does not cite this flower as being endangered or threatened; however, EPA expects DOE to consult with the Department of the Interior to ascertain the flower's status and what measures need to be taken to protect its critical habitat.

2. p. 3-71 to p. 3-73: EPA understands the sensitivity regarding the discussion of safeguards and security systems. However, heavy reliance upon electronic detection equipment may leave such systems vulnerable in case of a power failure. The assumption is that LLL has contingency plans for this circumstance but a confirmatory mention of this fact in the FEIS would assure that this possibility has not been overlooked.

3. p. 2A-14, Geologic History section, 23rd line: The term "clay" is incorrect, it should be "alluvium" or "siltstones and sandstones."

Enclosure 4

Congress of the United States
House of Representatives

RONALD V. DELLUMS
8TH DISTRICT, CALIFORNIA

DISTRICT OF COLUMBIA COMMITTEE

CHAIRMAN, SUBCOMMITTEE ON FISCAL
AND GOVERNMENT AFFAIRS

ARMED SERVICES COMMITTEE

ANY REPLY TO THIS LETTER
SHOULD BE ADDRESSED TO
OFFICE CHECKED.

WASHINGTON OFFICE:

☐ 2464 RAYBURN BUILDING
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(202) 225-2661

DISTRICT OFFICES:

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(415) 283-8125

☐ 2490 CHANNING WAY, ROOM 202
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DONALD R. HOPKINS
DISTRICT ADMINISTRATOR

April 11, 1979

STATEMENT FOR DEPARTMENT OF ENERGY HEARINGS, APRIL 12, 1979
RE: THE U.C.-LAWRENCE LIVERMORE SITE
DRAFT ENVIRONMENTAL IMPACT STATEMENT

I am deeply concerned by the Draft Environmental Impact Statement for the Livermore site, and submit this statement to share with you some of these concerns.

This DEIS is deficient in a number of areas, due primarily to the very narrow boundaries within which the Statement is framed. Because of this narrow focus, there is no serious consideration of a number of problems which could affect the health and safety of Bay Area and Northern California residents and have the potential to damage or destroy our local environment and perhaps more widespread areas.

The Statement fails, for instance, to address the problems of radioactive waste disposal in a comprehensive manner, only suggesting that site containment is adequate. What, however, will ultimately become of this waste? When the bulk of radioactive waste will be with us for several hundreds of thousands of years, surely it can be considered to have an environmental impact, and certainly the Department of Energy should be concerned with the proper, permanent disposal of waste from the Livermore site--should such disposal even prove feasible. In this regard, we should note the many problems with disposal outlined by the Interagency

Review Group on Nuclear Waste Management and the critique of their report by a number of state agencies.

The statement is also deficient in dealing with the consequences of a major release of radioactive materials and an attendant civilian emergency. The definition of D.O.E. responsibility in such a situation is defined in the narrowest of terms, leaving other governmental agencies (whether local, state, or Federal) to cope with the monumental impact of a major accident. My office learned recently, for example, that in one of the cities in the 8th Congressional District, there has been but one meeting of the Disaster Council for the last two years and that no effective plans exist for dealing with this type of nuclear disaster, despite the real risk posed by the University of California Laboratories. While we can hold local officials responsible for such negligence, certainly D.O.E. bears a prime responsibility in this matter to make certain that the public clearly understands the devastation which could ensue in the event of a major release of radioactive materials. The DEIS seems the appropriate arena to explore these consequences.

More immediately, however, I am particularly concerned by the recent discoveries of additional earthquake faulting in the Livermore Valley. As you may be aware, my office has been an intervenor in the N.R.C. hearings regarding the G.E. Test Reactor Vallecitos facility in the Livermore Valley, and we have seen that there are numerous earthquake faults which only recently have been mapped or properly identified for the potential intensity of a major seismic event. There is, in my estimation, an urgent need to undertake a thorough investigation of the faults on and near the Livermore Site. I understand that the Department of Energy agrees, and I want to take this opportunity to urge that this be a major independent study

of the Tesla and other faults in the area to determine the full extent of the danger they pose. If the Tesla fault is longer than currently mapped and does run beneath the laboratory facilities at Livermore, the implications are manifold for possible danger to the health and safety of Bay Area residents.

In the interim, there exists considerable danger in the form of large quantities of plutonium which are stored and used at the Livermore site. Were there to be a major earthquake that caused the release of this plutonium into the atmosphere, the consequences would, indeed, be catastrophic. In addition, other evidence presented here today indicates that there may already be a grave danger to the surrounding population from an increased risk of cancer. I request, therefore, that until such time as the potential danger of these newly discovered earthquake faults and other problems can be adequately assessed, all plutonium at the Livermore Site be removed to a storage facility or to another site removed from the threat of earthquake or other potential damage.

Sincerely,

Ronald V. Dellums
Member of Congress

RVD:ak

Natural Resources Defense Council, Inc.

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December 21, 1977

Natural Resources Defense Council
COMMENTS ON ERDA-1545-D
Draft Environmental Impact Statement
Rocky Flats Plant Site
Golden, Colorado

Arthur R. Tamplin
Thomas B. Cochran

Introductory Remarks

The DOE proposes to modify the Rocky Flats facility and to continue its operation for the production of nuclear weapons. The draft EIS assumes that this proposed action is beneficial and asserts that an analysis of the alleged benefits of the proposal is beyond the scope of the environmental analysis. This position is clearly erroneous, conflicts with established legal precedents, and is indefensible as a matter of policy.

The mandate of NEPA is clear -- in evaluating any proposed major federal action, the agency must thoroughly explore the alleged benefits of the proposed action and alternatives which will equally or better achieve the legitimate benefits sought. Absent preparation of a programmatic

impact statement on the United States nuclear weapons program, DOE is obligated to explore in this EIS the clearly relevant issues related to United States national defense and the role, if any, which Rocky Flats can play in that defense. Kleppe v. Sierra Club, 427 U.S. 390 (1976); Natural Resources Defense Council v. Nuclear Regulatory Commission, 547 F.2d 633 (D.C. Cir. 1976), cert granted 45 U.S.L.W. 3554 (Feb. 22, 1977). In addition, the existence of significant and authoritative criticism of the defense policy which is used as the unexplored premise of this draft EIS must be fully disclosed and addressed in the EIS. Committee for Nuclear Responsibility, Inc. v. Seaborg, 463 F.2d 783 (D.C. Cir. 1971); Save Our Ten Acres v. Kreger, 472 F.2d 463 (5th Cir. 1973); Environmental Defense Fund v. Corps of Engineers, 325 F.Supp. 728 (E.D. Ark. 1971). Finally, despite the fact that major commitments have already been made based upon the assumed validity of our present national defense policy, the present proposed action cannot be authorized without analysis of the programmatic issues involved. Scientists' Institute for Public Information v. A.E.C., 481 F.2d 1079 (D.C. Cir. 1973).

In the past the individuals preparing the various parts of an impact statement have done so in an atmosphere of anonymity. This, we feel, has materially contributed to the poor quality of the statements. The U.S. Code at 18 U.S.C. § 1001 states:

> Whoever, in any matter within the jurisdiction of any department or agency of the United States knowingly and willfully falsifies, conceals or covers up by any trick, scheme, or device

> a material fact, or makes any false, fictitious or fraudulent statements or representations, or makes or uses any false writing or document knowing the same to contain any false, fictitious or fraudulent statement or entry, shall be fined not more than $10,000 or imprisoned not more than five years, or both.

We therefore request that this state of anonymity be ended and that those who prepare the various parts of the FEIS be fully identified.

General Comments

We find this DEIS is seriously inadequate in a number of respects related to safeguards and health and safety. We shall comment on these subsequently. At this point, we shall comment on a major deficiency that makes the DEIS totally inadequate. It is a deficiency that again demonstrates the crabbed interpretation of NEPA by ERDA and it must be corrected in the FEIS or in a separate Environmental Impact Statement. This deficiency relates to the benefits of the Rocky Flats Plant and hence to the cost/benefit or risk/benefit analysis.

On page 1-4 of the DEIS it is stated, "The principal benefit from the Rocky Flats Plant is its contribution to national defense." On page 1-18, it is stated, "A complete benefit-risk analysis of the national defense program is beyond the scope of this Statement." The apparent basis for these statements appears on p. iv:

> The United States' current defense posture dictates the need for nuclear weapons. As a result, weapon production is a mandate of the Administration, Congress, and the Department of Defense. The production of nuclear weapons, in which the Rocky Flats Plant maintains a vital role, will probably continue for as long as the world situation suggests that this

country must have a strong defense. The present and future operation of the Rocky Flats Plant therefore cannot be divorced from America's defense needs.

The approach taken and arguments presented in this DEIS are quite similar to those taken by the AEC in the case of the Clinch River Breeder Reactor and the LMFBR Program. The courts have asserted that this approach was wrong (Scientists' Institute for Public Information v. A.E.C., supra). By accepting the doctrine that the U.S. national security dictates the need for any and all nuclear weapons and that the Rocky Flats Plant must maintain a vital role in their production, is simply an unacceptable crabbed interpretation of NEPA -- an interpretation that the courts have disallowed.

This crabbed interpretation, among other things:

- fails to consider the opinion of opposing competent authority;
- fails to consider whether continual production of nuclear weapons at Rocky Flats or elsewhere adversely affects our national security;
- fails to consider the alternative of operating Rocky Flats or elsewhere at a reduced throughput;
- fails to consider alternative approaches to national security, such as nuclear disarmament;
- fails to consider the impacts of the use of nuclear weapons;
- fails to consider the impact of our nuclear weapons program on the development and expansion of similar programs world-wide;
- fails to consider the impact of unilateral actions on our part with respect to reducing the nuclear weapons arsenal elsewhere;
- fails to consider the command and control problems associated with nuclear weapons -- problems that could result in unauthorized detonations and even

trigger a massive nuclear war;

fails to consider the problems associated with the deployment and possible use of tactical nuclear weapons;

fails to consider the sociological effects associated with the "balance of terror" philosophy;

fails to consider the problems associated with safeguards, including the invasion of privacy and the erosion of civil liberties.

These represent some of the issues raised by competent authorities. As representative, we offer Dr. Herbert York who was chief scientist for the Department of Defense throughout the Eisenhower Administration and into the Kennedy Administration. In his book, Race to Oblivion (Simon and Schuster, New York, 1970), Dr. York states on page 21:

> In January, 1961, I had the opportunity to discuss these matters with John J. McCloy, who was President-elect Kennedy's personal and principal adviser on matters of arms control and disarmament. I communicated to Mr. McCloy the substance of what I stated publicly before the Senate Foreign Relations Committee in 1963:
>
> > Ever since shortly after World War II, the military power of the United States has been steadily increasing; over the same period the national security of the United States has been rapidly and inexorably diminishing. . . . It is my view that the problem posed to both sides by this dilemma of steadily increasing military power and steadily decreasing national security has no technical solution. If we continue to look for solutions in the area of military science and technology only, the result will be a steady and inexorable worsening of this situation. I am optimistic that there is a solution to this dilemma; I am pessimistic only insofar as I believe there is absolutely no solution to be found within the areas of science and technology.

On page 23 Dr. York indicates that other DOD officials share his views and notes comments made by the present-day Secretary of DOD:

> By no means am I the only Department of Defense official who has come to realize the dilemma of an ever-increasing military power accompanied by an ever-decreasing national security. Nor am I the only defense official to realize that the dilemma cannot be resolved by the development and deployment of ever more complex and more costly machines. Harold Brown said after serving more than four years as DDRE and nearly four years as Secretary of the Air Force:
>
> > Those who have served as civilian officials in the Department of Defense at the level of Presidential appointment . . . have recognized the severely limited utility of military power, and the great risks in its use, as well as the sad necessity of its possession . . . [The] higher their position and, hence, their responsibility, the more they have come to the conclusion that we must seek national security through other than strictly military means . . . and urgently.

On page 91 Dr. York, drawing upon his long experience at DOD, states:

> Thus, the real reason that this year's defense budget is so and so many billion dollars is simply that last year's defense budget was so and so many billion, give or take about five percent. The same thing, of course, applies to last year's budget and the budget of the year before that. Thus the defense budget is not what it is for any absolute reason or because in any absolute sense the total cost of everything that is supposedly truly needed comes out to be precisely that amount, but rather it is the sum total of all the political influences that have been applied to it over a history of many years, and that have caused it to grow in the way that it has grown.

On page 103 Dr. York points out a serious consequence of this budget process:

> From the point of view of arms control and the arms race, these excesses in dollars and people also had serious consequences. The extra organizations and the extra people resulted in a larger constituency favoring weapons development. This larger constituency in turn strengthened those forces in the Congress "which hear the farthest drum before the cry of a hungry child," and consequently the whole arms race spiraled faster than before.

Concerning parity in the arms race, Dr. York states on page 169:

> Thus a balance of terror had been created such that neither side could conceivably survive a nuclear exchange no matter who struck first, and even fairly large deviations from strict numerical parity could not seriously upset the balance.

On page 228 Dr. York states the absurdity of our actions and processes:

> The actions and processes described in this book have led to two absurd situations.
>
> The first of these absurdities has been with us for some time, and has come to be widely recognized for what it is. It lies in the fact that ever since World War II the military power of the United States has been steadily increasing, while at the same time our national security has been rapidly and inexorably decreasing. The same thing is happening to the Soviet Union.
>
> The second of these absurdities is still in an early stage and, for reasons of secrecy, is not yet so widely recognized as the first. It lies in the fact that in the United States the power to decide whether or not doomsday has arrived is in the process of passing from statesmen and politicians to lower-level officers and technicians and, eventually, to machines. Presumably, the same thing is happening in the Soviet Union.

On page 233 Dr. York discusses the problems of command and control and how our actions obtain reactions:

> Can we rely on the Soviets to invent and institute the same kind of controls? What will happen as advances in our weapons technology require them to put more and more emphasis on the readiness and the quick responsiveness of their weapons? Do they have the necessary level of sophistication to solve the contradiction inherent in the need for a "hair trigger" (so that their systems will respond in time) and a "stiff trigger" (so that they won't fire accidentally)? How good are their computers at recognizing false alarms? How good is the command and control system for the Polaris-type submarine fleet now being rapidly, if belatedly, deployed by the Soviets? Is it fail-safe?

Finally, on page 239 Dr. York discusses unilateral actions by the United States:

> Just as our unilateral actions were in large part responsible for the current dangerous state of affairs, we must expect that unilateral moves on our part will be necessary if we are ever to get the whole process reversed.
>
> It may be beyond our power to control or eliminate the underlying causes of the arms race by unilateral actions on our part. Our unilateral actions certainly have determined its rate and scale to a very large degree. Very probably our unilateral actions can determine whether we move in the direction of further escalation or in the direction of arms control and, in the long run, nuclear disarmament.
>
> Conventional good sense urges us to keep quiet, to leave these matters to the experts and the technicians. My father, troubled by my repeated trips East to testify against the ABM, asked me, "Why are you fighting City Hall?" His metaphor is sound; the defense establishment is indeed our City Hall, and it can be depended upon to care for its own interests, whether or not these are the interests of the entire nation. If we are to avoid oblivion, if we are to reject the ultimate absurdity, then all of us, not just the current "in" group of experts and technicians, must involve ourselves in creating the policies and making the decisions necessary to do so.

The final paragraph of the above quotation corresponds to the mandate of NEPA. The preparation of impact statements only on selected parts of the nuclear weapons program is not adequate. NEPA requires an impact statement on the overall program. Unless this is done, the FEIS on the Rocky Flats Plant will be totally inadequate and unacceptable.

ific Comments

The headings in this section will refer to the chapter ings in the DEIS.

Emergency Plans

This chapter is quite misleading. It discusses plans, ological assistance groups, medical treatment and other s without in any way defining the value of these groups plans in mitigating the consequences of a major release the RFP.

It is essential that the statement discuss:

- The response time of such groups as IRAP;
- The actual function of the groups in terms of preventing or reducing exposure or contamination and the time scale involved;
- The nature of the possible medical treatment and its value in reducing effects;
- The number of victims that could be given treatment;
- In short, it is essential to justify the assertion that these plans have a significant value in reducing contamination, reducing exposure and mitigating effects.

Safeguards

This section is totally inadequate. It does not discuss:

- The threat size (internal and external) against which the system is designed;
- The severe limitations of material accounting which makes this an almost useless safeguard tool;
- The opinions of competent authorities who state that existing safeguards are inadequate;

- The social costs associated with the civil liberties implications of safeguards;
- The GAO reports critical of ERDA safeguards.

A detailed discussion of these factors is contained in the attached testimony of Thomas B. Cochran, "Nuclear Weapons Proliferation and Safeguards." This is intended as an integral part of our comments.

3.1.2 Radiological Impacts

This chapter seriously underestimates both the dose and effects from operational and accidental releases.

1. It presents only the annual dose when the important dose is the dose commitment over life time of the radionuclides in the biosphere.

2. The dose conversion factors used for Pu-239 in bone underestimate the bone dose by at least a factor of 10.

3. A non-conservative approach is used for determining the dose to man via the soil-plant route.

4. Recent evidence indicates that the BEIR Report estimate of the somatic and genetic effects of radiation were too low by at least a factor of 10. Hence the risk estimates used in the DEIS underestimate by a larger factor.

These factors are discussed in detail in the attached "Testimony of Natural Resources Defense Council, Re: Chapter IV." This is intended to be an integral part of our comments.

UNIVERSITY OF CALIFORNIA, BERKELEY

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DEPARTMENT OF PHYSICS | BERKELEY, CALIFORNIA 94720

December 15, 1978

W.H. Pennington
Mail Station E-201, GTN
Department of Energy
Washington, D.C. 20545

Dear Mr. Pennington;

I am writing in regard to the recently issued Draft Environmental Impact Statement for the Livermore Site (DOE/EIS-0028-D, Livermore Site, September 1978.) Having read this document I now wish to make some critical comments, raise some questions, and call for public hearings.

In addition to my work as a physics teacher and researcher on this campus I have, for a number of years, concerned myself with the activities of the Lawrence Livermore Laboratory: its connection with my university, its use of scientific people and knowledge and, most of all, its contributions to the nuclear arms race which threatens the very survival of humanity. I had not anticipated that the process of drafting and approving an EIS for this laboratory would involve issues as broad as these; but I was mistaken. The DEIS clearly and repeatedly asserts that the main benefit coming from the operation of this Laboratory is its contribution to the National Defense through its primary mission of nuclear weapons research and development. What is totally lacking, however, is any word about the costs or adverse impacts that are likely to follow from this activity. Thus, in a very central manner, the present DEIS is far out of balance and cannot be considered in compliance with the requirements of the law.

There is a large body of opinion and published literature, encompassing both technical and lay people, which holds that the continued development of nuclear weapons is actually increasing the likelihood of nuclear war, thus decreasing the security of the nation and threatening enormous destruction to the human and natural environment. This risk side of the risk-benefit analysis has been totally ignored in the DEIS. Here are a few of the questions which the DOE should answer in order to provide a reasonable beginning for the necessary public review and evaluation of these hazards:
1) What is the probability of nuclear wars, of various sizes, occurring ?
2) What are the likely adverse consequences of such wars, to the population generally and to the LLL area in particular ?
3) How do the above mentioned risks compare with other types of nuclear accident risks that have been much debated - from nuclear power plant accidents, from earthquakes, from sabottage and terrorism, etc. ?

I request that, in addition to providing authoritative government assessments of the questions I have raised, the Department of Energy hold adequate public hearings on this DEIS so that full public input to and scrutiny of such evaluations can be provided.

Sincerely yours, Charles Schwartz

Charles Schwartz, Professor of Physics

UNIVERSITY OF CALIFORNIA, BERKELEY

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BERKELEY, CALIFORNIA 94720

March 30, 1979

W. H. Pennington, Deputy Director
Office of Environmental Compliance and Overview
U. S. Department of Energy
Mail Station E-201
Washington, D.C. 20545

Dear Mr. Pennington;

This is in confirmation of our telephone conversation this morning regarding the public hearing on DOE's Draft Environmental Impact Statement on the Livermore Site (DOE/EIS-0028-D). I wish to be a "full participant" in these procedings, now scheduled for April 12, 1979, in Livermore.

I shall participate as an individual concerned citizen and also as a professional research scientist and teacher: I have for many years concerned myself with the activities of the Lawrence Livermore Laboratory, its connection with my university, its use of scientific people and knowledge and, most of all, its contributions to the nuclear arms race which threatens the very survival of humanity. Following is an outline of the presentation which I wish to make at this hearing:

A) Argument as to why assessment of the risks of nuclear war must be included in the EIS. This was the central point in my letter of December 15, 1978, criticizing the Draft EIS. The DOE"Staff Statement in Response to Comments Received"(dated March 1979) was totally inadequate in responding to this issue.

B) Search for answers to the questions posed in my December 15 letter:
1) What is the probability of nuclear wars, of various sizes, occurring ?
2) What are the likely adverse consequences of such wars, to the population generally and to the LLL area in particular ?
3) How do the abovementioned risks compare with other types of nuclear accident risks that have been much debated - from nuclear power plant accidents, from earthquakes, from sabotage and terrorism, etc. ?

C) Follow-up inquiries from A) and B) and other related matters in the DEIS.

As I told you, I shall attempt to bring in some expert witnesses to assist me in presentation of these matters; and I also requested that DOE provide appropriate experts from within the government and the Laboratory to provide authoritative responses to the questions indicated. I suggested specifically a) the Director of LLL, the Associate Director for Nuclear Weapons at LLL, or some designee of theirs who is expert in nuclear plans and strategy and consequences of nuclear warfare; b) from DOE, the Director of the Division of Military Application, General Bratton, or his designee; and c) from DoD, an official of the Military Liaison Committee, which links the two agencies. I have a substantial amount of information and analysis to present and estimated that I would need about 1½ hours to make my own presentation with an equal amount of time needed in addition for the witnesses.

Sincerely yours, Charles Schwartz

Charles Schwartz, Professor of Physics

Should a public university continue to run the nuclear weapons laboratories for the U.S. government? The issue has shaken the University of California at Berkeley.

CHARLES SCHWARTZ

The Berkeley controversy over nuclear weapons

The University of California is the *alma mater* of every single nuclear weapon in the U.S. military arsenal. Since the dawn of the Atomic Age, this great university has been the government's trusted administrator of two unique laboratories whose mission has been to develop ever-improved warheads for all the nation's strategic and tactical nuclear weapons.

Because these weapons labs at Los Alamos (N.M.) and Livermore (Ca.) are far removed from the campuses of the University of California, it is only under very special circumstances that this strange alliance between the forces of learning and the forces of mass destruction becomes an issue of prominent concern. In 1969, amidst student riots and tear gas, the U.C. faculty set up a special group, the Zinner committee, to review the appropriateness of the university's involvement with the weapons labs. Their report generally approved the status quo, suggesting some slight changes in administrative relations, but was effectively ignored by the powers that be.[1]

In 1977, with the university's five-year contract for running the weapons labs up for renewal, a group of peace activists from the Berkeley campus and the surrounding community organized to bring public attention to this issue once again.[2]. The U.C. Board of Regents quickly approved renewal of the contract in spite of the group's call for a thorough and public review of the university's involvement; but, bowing to the pressure, U.C. President David Saxon did appoint a new committee to study the question once again. Whatever the preferred choices of the Regents, President Saxon or his appointed committee, it seems fairly clear that the questions being raised will not be easily pushed aside. The organized opposition to the continuing arms race is well-informed, dedicated and growing.

University and weapons lab officials have justified their common enterprise in the following way. Although the product is for military use, they say, the work at the laboratories is under civilian direction through a federal agency (originally the Atomic Energy Commission, then the Energy Research and Development Administration and now the Department of Energy). The projects undertaken at the labs, we are told, are in response to requirements originating in the Pentagon and approved by the President and the Congress. Thus, the weapons program is part of our national policy, as determined by democratic governmental process, and the university is fulfilling a valuable public service.

If this description of the labs' function is accepted, there still remain serious questions of basic moral responsibility as well as questions of academic appropriateness with regard to the university's role in developing weapons of mass destruction. However, the case that I will make here is that this description is more myth than reality: that there is practically no control over the labs other than the military; that there is very little effective review of the weapons programs by the democratically responsible people in either the executive or legislative branches of our government; and that the university, by playing the silent partner in this arrangement, has committed a grave disservice to the people of this nation.

While this indictment will be presented in an academic fashion (using quotations from official sources and establishment figures), the context of this criticism is, of course, something much larger than an academic study. The nuclear arms race is a world-threatening reality. It has reached the point where a good many sober observers are alarmed at the growing instabilities, created by the new generations of nuclear weapons systems, which point toward a potential for disastrous nuclear war. The weapons laboratories also are not academic institutions; they epitomize "science with a purpose." Our task is to examine them to understand whose purpose they serve.

Let's go back to 1945, when many of the scientists who had been involved in developing the atomic bomb joined with liberal political forces to fight, in the Congress, against a bill that would have put future control of all atomic energy directly in the hands of the military. In what was hailed as a great liberal victory, they finally obtained passage of the Atomic Energy Act of 1946, which put all control of atomic research and materials under a new civilian agency—the Atomic Energy Commission. As far as the weapons program was concerned, however, this was no victory at all, since the law specified that all military aspects of atomic energy would be under the direction of a military officer installed within the AEC framework. As one Pentagon official noted at the time, the new legislation "guarantees greater military participation" than does the original bill.[3]

As the AEC changed into ERDA which became part of DOE, this arrangement remained intact. A general, with his military staff, selected by the Pentagon, directs the military applications of atomic energy from

an office in a nominally civilian agency. When budget time comes, it is this military officer who presents to the Congressional committees that portion of the agency's budget under his command; the civilian DOE officials merely sit on the sidelines. Even though not all of the work at Livermore and Los Alamos is on weapons, when controversy arises relating to the laboratories it is the military officer who makes the decisions.

Recently a consortium of universities in New Mexico, Colorado, Utah, Wyoming and Nebraska proposed to ERDA that they might replace the University of California as contractor for running the Los Alamos Scientific Laboratory. Since the Los Alamos lab was assuming the role of a regional research center for energy problems, it was felt that people from the affected area should be involved in the management of the lab. This proposal was summarily rejected by General Alfred Starbird, ERDA's administrator for national security, who let it be known that he would rather see the labs abandon all non-weapons research than have them operated by anyone other than the University of California.[4] On other occasions, General Starbird has made it clear that weapons work must remain the major task of these laboratories, counteracting a recent trend toward diversification of the research programs at these major national research facilities.

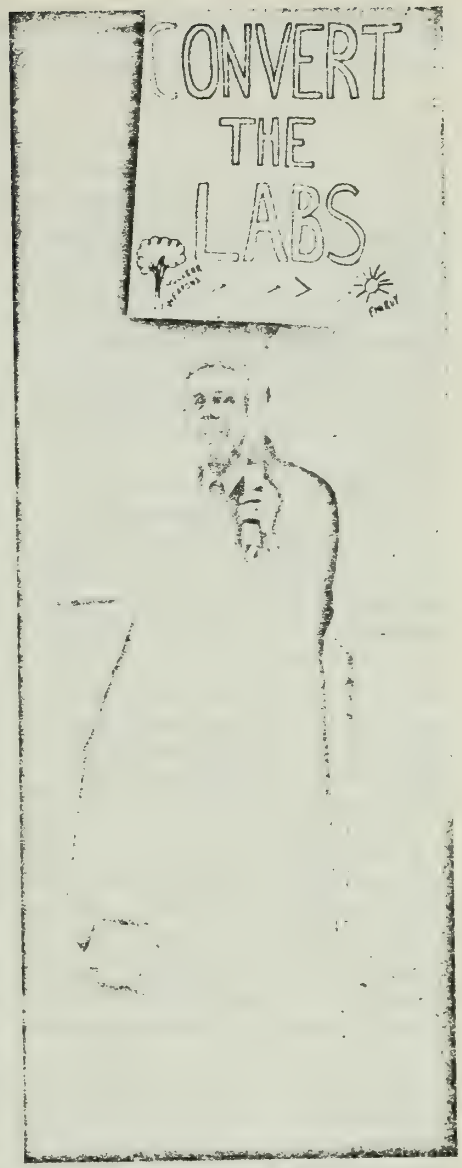
The military aura at the laboratories manifests itself not only in the pervasive security rules and in the strictly hierarchical management but extends to some of the most precious aspects of academic life. Lab staff members who also teach part time in the Department of Applied Science at Livermore must submit their requests for sabbatical leave to General Starbird for approval or rejection.*

Now let's examine the claim that weapons programs at the labs respond to requirements set out by the Defense Department and approved by the President and the Congress.

We have a body of evidence that shows the lab officials playing a very active political role, not just a passive technical role, in shaping the weapons policies in Washington. Edward Teller and his associates at Livermore lobbied strongly against the Nuclear Test Ban Treaty of 1963; and just recently, Livermore director Roger Batzel was again in Washington to testify against the Comprehensive Test Ban Treaty now being worked out between the United States and the Soviet Union. In 1976 the Livermore management team, apparently not satisfied with the effectiveness of their inside connections in Washington, decided to take their case directly to the people, calling a press conference to boost their recommendations for an expanded weapons development budget. This self-serving maneuver was criticized by the local press; and Congressman Pete Stark, who represents the Livermore district in Washington, denounced the Livermore officials as "grubbing for money" in the halls of Congress.[5]

With the Comprehensive Test Ban Treaty near at hand, lab managers are busily boosting other technologies which will let them continue weapons tests and development work on a laboratory scale. Chief among these is laser fusion, which

*General Starbird has now resigned. Duane Sewell, a Livermore official who has been at the lab since its founding, has been named to succeed him.



the Livermore lab glowingly publicizes as a long-term solution to the country's energy problem. However, as one Pentagon official admitted, "that's something that came along only after energy research got popular . . . really, this is a military program and it always has been."[6]

One of the most revealing stories came out of the recent publicity over the neutron bomb. Last summer the U.C. Nuclear Weapons Lab Conversion Project released evidence that showed the U.C. labs had not only designed this new weapon but promoted it. In 1973 Congressional testimony Harold Agnew, director of Los Alamos, spoke about this new weapon idea which we now know as the neutron bomb. Asked why this weapon had not been more fully

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exploited for battlefield use, Agnew responded,

> I really don't know why people have not thought more on the use of these [deleted] weapons.
>
> It may be that people like to see tanks rolled over rather than just killing the occupants. . . .
>
> I know we at Los Alamos have a small, but very elite group that meets with outside people in the defense community and in the various think tanks. They are working very aggressively, trying to influence the DOD to consider using these [deleted] weapons which could be very decisive on a battlefield, yet would limit collateral damage that is usually associated with nuclear weapons.[7]

I was unable to find out, from the university, anything more about this "elite group" at Los Alamos that lobbied "very aggressively" for the neutron bomb. Probably most of their work is classified. However, I was able to find a paper that appears to be one of their more public handiworks, published in the foreign policy journal *Orbis* in the summer of 1973. Entitled "A Credible Nuclear-Emphasis Defense for NATO," this article lays out the political and military rationale for a change in NATO strategy. While not mentioning the then classified words "neutron bomb," the article is clearly talking about the "discriminate use" of tactical nuclear weapons with reduced "collateral damage." The article was authored by three staff members of the Los Alamos Laboratory. What especially caught my eye was the information about former positions held by these Los Alamos weapons experts: one had served as Director of Nuclear Planning for NATO and a second was a retired U.S. Army colonel formerly in command of the Army Nuclear Agency.

The interchange of officials between the laboratories and other parts of "the defense community" is common. Three of Livermore's earliest laboratory directors went on to be chief of the Pentagon's research and development work.[8] The following story of a fourth director shows how lab management and government policymaking are intertwined.

Michael May was director of the Livermore lab from 1965 to 1971. In 1970 he published an article, also in *Orbis*, analyzing the deterrence theory of our strategic nuclear weapons force, and advocating a change to a counterforce strategy—one designed to fight and win a nuclear war. Many of the detailed arguments given by May in this early article turned up again in 1974, when Secretary of Defense James Schlesinger announced the shift in U.S. strategic policy toward a counterforce doctrine. Within the past few years May has had the opportunity to carry his ideas into practice, serving as senior personal advisor to the Secretary of Defense for SALT and then as a member of the U.S. delegation at SALT. He is now once again at Livermore as associate-director-at-large.

May is not the only weaponeer involved in the SALT talks; a number of people from the labs are involved as technical experts advising the negotiators. But the problem is that people who have both political and bureaucratic interests in pushing weapons are not the ones who should be influential in negotiations that are intended to put them eventually out of business. The former chairman of the General Advisory Committee to the U.S. Arms Control and Disarmament Agency—the group that helps to shape the President's approach to SALT—is none other than the director of the Los Alamos lab, Harold Agnew.

Now we turn to the claim that the weapons programs of the laboratories, regardless of how they may be initiated, must be approved by the Congress and the President. Of course there is a formal budget approval process. The question is, how thorough is the review and evaluation by people outside of the weapons complex?

When the neutron bomb story hit the headlines, we were told that neither the President and his staff nor most members of Congress were even aware of the existence of this item in the budget. But that is just one weapon. What about the many others?

An article published in *Science* in June 1977 tells about a procedure established two years ago in which various parts of the Executive branch—DOD, ERDA, ACDA and the National Security Council—were supposed to review every new weapons proposal and draw up a detailed "arms control impact statement" providing critical information about the impact the proposed weapon might have on the stability of the arms race.[9] The actual reports turned out to be so superficial that angry legislators called them "totally useless," a "farce," and a "mockery."

The following remarks, made by Senator Stuart Symington at the opening of some subcommittee hearings in 1973, convincingly tear down the myth that the Congress has been able to exercise effective review of nuclear weapons programs. Senator Symington was, by the way, not a softy on military matters; he served as Secretary of the Air Force under President Truman.

> This is the beginning of what we hope will be an informative and constructive series of hearings on the military applications of nuclear technology. Some have heard me state previously, not until I became a member of the Joint Committee and traveled to Europe with Senator Pastore in the spring of 1971, did I realize the true military strength of the United States and became acquainted with the vast lethal power of our nuclear arsenal . . . . I actually learned more about the true strength of the U.S. forces in Europe in those six days than I had in some 18 years on the Armed Services Committee . . . . One cannot help

The university provides an aura of academic legitimacy to the business of weapons development.

but consider the implications incident to our defense and foreign policies if these facts were known by the appropriate committees of the Congress, as well as in more general fashion by the American people.[10]

Another quotation deals specifically with the role of the nuclear weapons laboratories. It was found in the 1974 report of the Scientific Advisory Committee set up by U.C. President Charles Hitch to review the work of the labs. In discussing the Primary Laboratory Mission, this report states: "The established policy of the U.S. government is unequivocal concerning the necessity for continued nuclear weapons research and development." And it cites, as an official statement of this government policy, the following sentence from a Congressional report:

> The committee fully recognizes and supports the invaluable role of AEC's weapons laboratories, testing installations and production facilities in the development of a credible nuclear deterrent for the United States.[11]

I thought I should check into this reference and see what was the context from which this sentence of Congressional approval was gleaned. Was it indeed an "unequivocal" policy statement based on a thorough evaluation of the weapons complex; or was it something else? What I found was this: a recommendation from the Appropriations Committee that the Weapons Testing Program budget be slightly reduced and one project—for Artillery Fired Atomic Projectiles—be cancelled. The laudatory sentence cited above was tacked on as nothing more than a consolation prize, a pat on the head; indeed it seems almost like an apology from the Congress for not giving the weaponeers 100 percent of what they wanted.

Putting these several pieces together, the overall picture of the U.C. nuclear weapons laboratories that emerges is as follows:

- Whatever outside control there is over the labs is by military, not civilian officials;
- There is a substantial interchange of upper level personnel between the laboratories and the military establishment;
- Laboratory managers take an aggressive political role in promoting not only particular weapons systems but also basic changes in military strategy and national policy connected thereto; they even play a direct and commanding role in international negotiations on arms control;
- The degree of oversight exercised by our national leaders over the weapons program seems to be, at best, superficial; and
- The amount of information provided the general public is kept to an absolute minimum. It thus seems correct to conclude that our nation's nuclear policy has become the private property of the weapons technocrats. We all know of President Eisenhower's warning, in his farewell address, about the power of the military-industrial complex. Less well known is a second warning which Eisenhower made in that same speech:

> Yet in holding scientific research and discovery in respect, as we should, we must also be alert to the equal and opposite danger that public policy could itself become the captive of a scientific-technological elite.

That prophetic vision of yesteryear has become the entrenched reality of today. The weapons labs roll on as quite autonomous institutions, free of the impediments of democratic government. We, the people, not only pay for this enterprise with our taxes but we also suffer the growing threats to our very survival posed by the uncontrolled momentum of the nuclear arms race.

Under the patronage of the U.C. Board of Regents, with the cooperation of the U.C. president's office, and with the consent of the faculty, the University of California has been the nominal administrator of the two nuclear weapons labs under contract from AEC/ERDA/DOE. The major features of this relationship have been described by various people. Edward Hammel, an assistant director at Los Alamos, has made the following pointed remarks:[12]

> With respect to U.C. "management" of LASL operations, U.C. plays the role of a "benevolent absentee landlord." The "business" of LASL is for the most part with Washington. U.C. understands that and interferes in program not at all.

The major benefits to the labs from the university connection are, according to Hammel, "prestige," in that the U.C. name helps "in the recruitment and retention of scientific personnel"; and "independence," in that the laboratory staff "enjoys a much greater degree of freedom in its interactions with government officials than would be the case" were they under some government or industrial management.

That this "freedom" which the University of California grants to the lab managers may not be such a good thing was one of the sharpest criticisms made in the faculty's Zinner Committee report: "We are particularly disturbed by the nominal leadership which the University provides. The laboratories enjoy a delightful autonomy within the protective shelter of the University, so delightful as to border on the licentious." That committee recommended, and the faculty approved by overwhelming vote, a continuation of the university's relationship with the labs only with modifications designed to have the University exercise some responsible guidance over the operation of the laboratories.

That noble intention has been a total failure. In the opinion of some people, including a number of scientists working at Livermore, the last

The laboratories are something like Frankenstein's monster; it is no good trying to wash our hands and walk away.

five years has seen an increase in the isolation of the laboratories from meaningful contact with the university community, and an increase in the autocratic character and behavior of the labs' top management. For example, one of the major recommendations of the Zinner Committee was that the laboratories prepare annual plans for their unclassified activities and have these plans reviewed by campus authorities. When asked by the U.C. president's office, in 1976, what steps he had taken to implement this recommendation, LASL director Harold Agnew replied:

> These plans are reviewed and approved by the appropriate ERDA Headquarters Program Managers, and form the basis for funding of the Laboratory. While we will be happy to provide you with copies of these plans, it is not likely that it will be acceptable to ERDA to make any changes of substance.[13]

Thus the university provides an aura of academic legitimacy to the business of weapons development and provides the laboratory management with a two-sided carte blanche: they are free of any supervision from within the university and yet the university name gives them independence from any other source of control.

What the university has given to the weaponeers is more than freedom, it is license: license to promote their own bureaucratic and political interests with an absolute minimum of accountability to the democracy that they claim their work is intended to protect. I conclude that the University of California has thus done a very grave disservice to the people of this nation, and of the world, by its involvement in the nuclear weapons business.

The greater sins are probably not what the university *has* done in this connection, but what it *has not* done. It has not taken a responsible part in the management of the labs. It has not worked to decrease secrecy and disseminate knowledge about the weapons program. It has not fostered research that might be critical of the military establishment nor has it encouraged education about the vital issues of the arms race. And when some concerned individuals—faculty, students, people from the community—take the initiative and try to generate public education and serious study and debate about these issues, the University of California goes to painful lengths trying to maintain the curtains of darkness. Last November U.C. President Saxon ordered the arrest of six persons who had been quietly sitting in his outer office for 30 hours asking him to provide some official who would meet with them in a public debate about the nuclear weapons labs.[14]

With continued public attention being given to the weapons labs, it is likely that much of the university's hierarchy is finding the subject a bit too embarrassing and would like to pull out of the weapons business. But for the university to pack up and get out, in the name of academic purity, would indeed be irresponsible at this point. A great deal of harm has already been done. The laboratories are, in more than one sense, something like Frankenstein's monster; it is no good trying to wash our hands and walk away.

Of course the university cannot solve this problem by itself. The military interests are deeply entrenched and a major political fight in Washington will be needed to bring about any substantial change. There is a significant role that the university could play in this struggle, using its educational resources and its prestige. But we know better than to rely on the university officials for leadership: they have disappointed us too many times before.

We need to mount a political campaign on our own. We need to arouse our fellow citizens to the dangers of the arms race and its promoters. We need to build a constituency of people who understand that democracy dies when decisions are handed over to technical experts, or hidden behind a veil of secrecy, or sold upon hysterical exaggerations of some external threat. We need to work to bring the weaponeers under strict democratic control here at home before we can expect to make any serious progress in reducing the threat of nuclear arms by agreements between nations.

Notes

1. This committee was chaired by Paul Zinner, a professor of political science at the university. Professor George Wetherill, a minority of one, argued that the university should not be in the nuclear weapons business. In a subsequent ballot of the entire faculty, 43 percent voted to sever the U.C.-lab ties.

2. The U.C. Nuclear Weapons Labs Conversion Project (UCNWLCP), 608 Eshleman Hall, U.C., Berkeley, Ca. 94720. The author has been an active member of this group since its inception.

3. See R. G. Hewlett and O. E. Anderson, *A History of the USAEC, Vol. I, The New World, 1939/1946* (University Park, Pa.: The Pennsylvania State University Press, 1962), p. 515.

4. See *Science*, May 13, 1977, p. 743.

5. Livermore *Valley Times*, Aug. 10, 1977; Livermore *Independent*, Dec. 1, 1976.

6. Major General Edward B. Giller (General Starbird's predecessor in ERDA), quoted in *Science*, April 4, 1975, p. 30.

7. "Military Applications of Nuclear Technology," Hearings, Subcommittee on Military Applications of the Joint Committee on Atomic Energy, April 16, 1973, Part I, p. 49. See also J. K. Miettinen, "Enhanced Radiation Warfare," *Bulletin*, Sept. 1977.

8. They are Herbert F. York, Harold Brown (now Secretary of Defense) and John S. Foster, Jr.

9. *Science*, June 10, 1977, p. 1181.

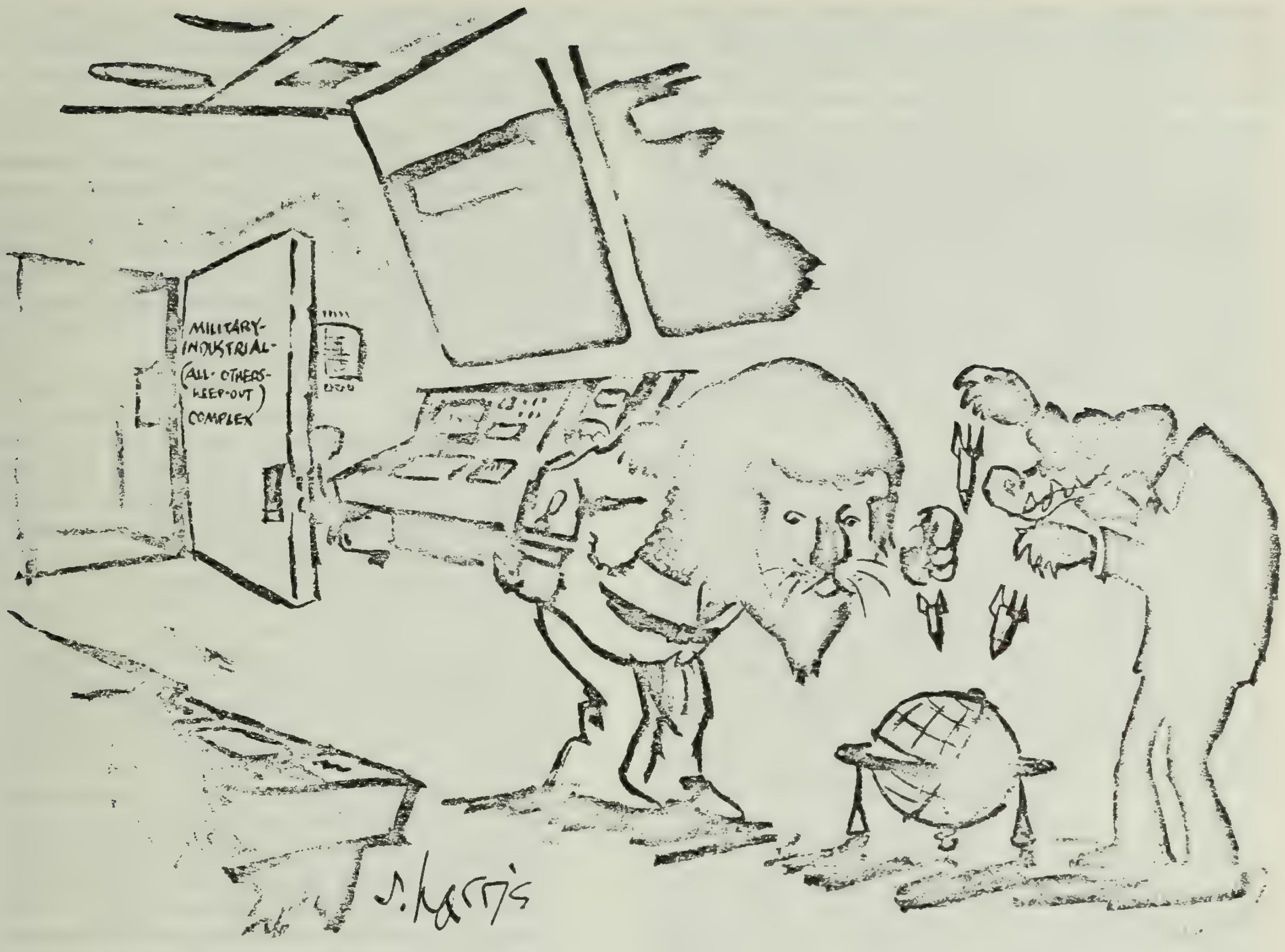
10. See n. 7, p. 1.

11. "Appropriation Bill for Public Works, Water and Power and Atomic Energy for the Fiscal Year 1974," HR 93-327, House of Representatives Committee on Appropriations, June 25, 1973, p. 6.

12. LASL office memo, July 18, 1977, with attachments.

13. Attachment C., *Items for Discussion*, Regents Meeting, Feb. 17, 1977; attachment 3-2.

14. The author and five other members of the UCNWLCP, charged with the misdemeanor of trespassing, were acquitted after an 8-day trial in the local court. One juror, quoted in the press after the verdict was announced, said, "They had the wrong people on trial."



Helping the Pentagon Aim Right

A report on the Scientific Target Planning Staff

CHARLES SCHWARTZ

While the overall questions about nuclear war—the size, capability, threat and doctrine of the U.S. and the USSR nuclear arsenals—have been subjects of continual public discussion, the details of military plans associated with the possible use of these weapons have been, understandably, closely guarded secrets. Within recent months, however, the U.S. Secretary of Defense, James Schlesinger, has broached a public discussion of significant shifts in policy: more U.S. nuclear missiles would be targeted on Soviet military and industrial targets rather than on cities, and efforts would be made to increase the accuracy and explosive power of the warheads.

Debaters aroused over this issue may, for convenience, be classified as doves or hawks, with the following definitions: the primary objective of doves is to assure that war will not occur; the primary objective of hawks is to assure that if war should occur, the United States will win it.

In discussions of nuclear warfare policy doves and hawks are associated with countervalue and counterforce strategies, respectively. Doves argue that countervalue strategy (targeting our missiles

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against cities as a threat of retaliation) is that which deters the other side from starting a nuclear war. Hawks argue that counterforce strategy (targeting our missiles against military and industrial installations) is needed to minimize our own losses in the event that nuclear war should break out. Doves oppose a counterforce strategy on the grounds that aiming our missiles at military sites, particularly with increased accuracy and payload, makes it look like we are striving for a first-strike capability, and that this can seriously destabilize the present balance of deterrence. Hawks oppose a countervalue strategy on the grounds that the doctrine of retaliation against civilian populations is immoral and may not be a convincing deterrent against small scale nuclear attacks.

These two points of view have been in contention for as long as there have been nuclear missiles. The hawks have been dominant inside the Pentagon and inside the circles of private industry which are closely allied with weapons development and manufacture; the doves have been dominant in the academic community. The Arms Control and Disarmament Agency has been perhaps the most responsive arm of the federal executive as far as the doves are concerned, but ACDA's power is hardly a match for that of the Department of Defense. Members of Congress may be found aplenty on both sides of this debate.

One might ask the question, Why has Defense Secretary Schlesinger now launched this campaign for a switch toward the more hawkish nuclear missile policy? It seems, however, that part of the shift in policy—the targeting on military sites in the USSR—has already taken place; the other part—increased accuracy and payload—is still being debated in Congress. This article will attempt to provide some answer to the question stated above, going beyond the theory of the bargaining chip approach to SALT.

Target Planning

A new slice of information relating to nuclear targeting policy has recently come into the public domain. At the end of 1972 Congress passed the Federal Advisory Committees Act (PL 92-463) which requires annual publication of information about all the many advisory committees that are appended to the federal executive branch. The "First Annual Report of the President to the Congress" on "Federal Advisory Committees"—a four volume, 5,703 page compilation—was issued early in 1973 by the Committee on Government Operations of the U.S. Senate. On pages 1,315 to 1,318 of this document, we found the report of the Scientific Target Planning Staff (JSTPS SAG) for calendar year 1972. We learned there that this advisory committee was originally established on May 1, 1968 by the Joint Chiefs of Staff (JCS), with the approval of the Assistant Secretary of Defense for Administration, and has the following stated function: "To provide timely technical and scientific advice of qualified scientists to the Director of Strategic Target Planning during the development of the Single Integrated Operational Plan." The committee met twice during 1972 (for three days in March and three days in September) and minutes of these meetings were submitted to JCS as "Top Secret Restricted Data." The committee membership is also listed.

That is not a great deal of information but it is certainly enough to arouse one's curiosity. The name "Single Integrated Operational Plan" (SIOP) was previously unknown to this author; but it appeared shortly in two other public reports. In an article, "The President and the Plumbers: A Look at 2 Security Questions," in the New York Times on Dec. 9, 1973, Seymour M. Hersh wrote,

> . . . the White House feared that Dr. Ellsberg . . . was capable of turning over details of the most closely held nuclear targeting secrets of the United States, which were contained in a highly classified document known as the Single Integrated Operations Plans, or SIOP. . . .
>
> The SIOP was a joint services nuclear targeting document that had been drafted—under the aegis of the Air Force's Strategic Air Command—in the late 1950s. The plan combined all the nuclear targeting options of the individual military services into a computerized program with centralized control.
>
> In essence, qualified sources said, the SIOP project controls the timing and attack patterns of American nuclear bombs that would be released from Army, Navy and Air Force strategic aircraft, missiles and submarines. It also includes specific targeting information for every significant military objective inside the Soviet Union and China, including the number and power of nuclear warheads programmed for each objective.
>
> In the early days of the Kennedy Administration, the SIOP was drastically revised and provided with a limited series of nuclear options, the sources recalled, giving the President at least the flexibility to attack either the Soviet Union or China, and not necessarily both, as an earlier SIOP called for.
>
> Dr. Ellsberg, the source said, was active in working on the revised nuclear targeting plans under then Secretary of Defense Robert S. McNamara. Well-informed sources said that although the plan had been updated and altered, Dr. Ellsberg's information could have been extremely compromising to national security.

The other report mentioning SIOP is entitled "U.S. Security Issues in Europe: Burden Sharing and Offset, MBFR [mutual and balanced force reduction] and Nuclear Weapons," a staff report prepared for the Subcommittee on U.S. Security

A new slice of information relating to nuclear targeting policy has recently come into the public domain as a result of the 1972 Act which requires annual publication of information about all advisory committees.

No member of the Scientific Advisory Group comes from a university or is someone who is identified with the arms control community. Every member is associated with institutions known for their 'pro-armaments' views.

Agreements and Commitments Abroad of the Committee on Foreign Relations, U.S. Senate, dated December 2, 1973. This study deals mainly with the role of tactical nuclear weapons which the U.S. has deployed in Europe; and it discusses how those local issues are tied to larger plans for strategic nuclear war between the United States and the Soviet Union. That part of the report relating to SIOP (p. 21) reads as follows:

> The second major category of nuclear warfare for which the GSP (General Strike Plan) provides is general nuclear response which contemplates massive nuclear strikes against targets [deleted] in order to [deleted]. The general nuclear response would not be undertaken by theatre nuclear strike forces alone but only in conjunction with the execution of the Single Integrated Operations Plan (SIOP), the U.S. Joint Chiefs of Staff plan for the widespread synchronized use of U.S. nuclear weapons in an all-out war. Accordingly, responsibility for carrying out NATO's general nuclear response falls on U.S. strategic forces based outside Europe.
>
> Targets to be struck in the general nuclear response are part of what is known as the "Scheduled Program" and include the Priority Strike Program (PSP) and the Tactical Strike Program (TSP). The PSP is comprised of those strikes of highest priority to SACEUR [Supreme Allied Command Europe]. They include [deleted]. Another group of targets covered by the PSP are those included in the Allied Commander Europe's Critical Installation List. There are approximately [deleted] targets on the list of which theatre nuclear forces would strike about [deleted] under the PSP [deleted]. The TSP is a list of about [deleted] nuclear strikes against [deleted]. These are [deleted]. All PSP and TSP strikes have been coordinated, or "deconflicted," with the U.S. SIOP target list maintained by the Joint Strategic Targeting Center at Omaha.
>
> We were told by Defense Department officials that a general nuclear response might be required under two contingencies. The first would be [deleted]. The second would be [deleted].

One can only imagine what the deleted portions tell about targeting plans. However, it is already apparent from the two descriptions given above that SIOP is considerably more than just the retaliatory plan for a purely countervalue deterrent strategy: the references to military objectives and priority lists of targets clearly relate to a counterforce strategy.

Scientific Advisory Group

The new information which we have to present concerns the Scientific Advisory Group which has been working on SIOP. The report on JSTPS SAC, which we cited earlier, gives the names and addresses of the 13 members of this advisory committee. We have gathered whatever information we could find from open sources in an attempt to form a picture of the political-philosophical complexion of this group. The results are presented below.

In summary, committee membership divides into three nearly equal categories: members are from (1) inside the government, (2) private industry, or (3) defense-contracted think tanks and weapons laboratories. The government people are all employed directly by the Department of Defense; two are in the military and three in civilian positions. The four industry representatives are all from very large firms in aerospace and electronics that are doing major amounts of business with the Department of Defense; two of them have previously held administrative posts in the government. The last four members are from organizations which, while they are not major partners in the military-industrial axis, have long been intimately tied to the Pentagon.

No member of this committee comes from a university or is someone who is identified with the arms control community. Every member is associated with institutions known for their "pro-armaments" views. We conclude that this advisory committee working on SIOP is overwhelmingly, if not totally, populated by hawks and super-hawks.

The purpose of advisory committees, according to the Federal Advisory Committees Act, is to furnish "expert advice, ideas, and diverse opinions" to government agencies. The committees are to make use of advisors from outside the government and, according to the Act, membership should be "fairly balanced in terms of the points of view represented and the functions to be performed." The executive order written to implement this Act says, further, "the membership of a committee whose sole function is to consider scientific questions may be limited to scientists. However, an effort should be made to include scientists representing different points of view and different types of employment (university, industry, etc.)." Thus, one could claim that this particular committee was illegally constituted.

The principal aim of this paper is to try to understand the new shift toward the counterforce nuclear strategy. If our attempt to sort out the mechanisms and the motives for this important change in nuclear policy rests in part on speculation, that is because the government and, in particular, the Department of Defense so habitually conceals the truth from the citizenry.*

It is clear that SIOP is the pivotal blueprint for total nuclear war, certainly the most awesome docu-

*A request for information about the Scientific Advisory Group was sent to the agency representative in charge of this committee, whose name and address were given in the published report cited above. No response was received.

1972 Members of the Scientific Advisory Group to the Joint Strategic Target Planning Staff

Lt. Gen. Glenn A. Kent, USAF. Director, Weapons Systems Evaluation Group, Arlington, Va. (formerly Assistant Chief of Staff, Plans and Analysis, Air Staff).

Vice Adm. Robert L. J. Long, USN. Commander, Submarine Forces, U.S. Atlantic Fleet, Norfolk, Va. (formerly executive assistant to Under Secretary of the Navy; deputy commander, Naval Ship Systems Command).

Peter H. Haas. Scientific Assistant to the Deputy Director (Science & Technology) of the Defense Nuclear Agency, Washington, D.C. (formerly with the Defense Atomic Support Agency and with the Army's H. Diamond Labs).

Charles M. Johnson. Deputy System Manager for Science & Technology, Safeguard System Office, Arlington, Va. (He has held this position in the Army's ABM project since 1967.)

Nils F. Wikner. Special Assistant, Net Technical Assessment, Defense Research & Engineering, Washington, D.C. (formerly Deputy Director, Defense Atomic Support Agency; Special Assistant, for Threat Assessment, under Director of Defense Research & Engineering).

James R. Burnett. Vice Presisent, TRW Systems, Redondo Beach, Cal. (TRW sales in 1972 amounted to $1.68 billion, mostly in aerospace and electronics manufacturing; 22 percent from U.S. government.)

Welko E. Gasich. Corporate Vice President and General Manager Aircraft Division, Northrop Corporation, Hawthorne, Ca. (Northrop sales in 1972 amounted to $574 million, with the aircraft division accounting for 45 percent of this; 56 percent of the company's business is with the U.S. military.)

Fred A. Payne, Jr. (SAG Chairman) Vice President, Technical Operations, Martin Marietta Corporation, Orlando, Fla. (Martin Marietta sales in 1972 amounted to $1.04 billion; 52 percent of this was in aerospace manufacturing, the bulk of which is under U.S. government contracts. Formerly, Payne served as Deputy Director, for Strategic & Defense Systems, under the Director of Defense Research & Engineering.)

Joseph F. Shea. Senior Vice President and General Manager, Equipment Division, Raytheon Co., Whalen, Mass. (Raytheon sales in 1972 amounted to $1.46 billion, manufacturing in electronics and communications; 48 percent of sales to U.S. government. Formerly, Shea worked for TRW and for NASA.)

Arthur T. Biehl. Vice President, Research & Development Associates, Inc., Santa Monica, Ca. (Formerly an Associate Director at the Lawrence Livermore Laboratory (LLL); Biehl left this major weapons laboratory to help found the private Defense think tank, R&D Associates, in 1971. This new organization was formed chiefly by the [illegible] physics department of the Rand Corp., a group with [illegible] connections within Department of Defense.)

Charles A. McDonald. Associate Director [illegible] Applications, Lawrence Livermore Laboratory, [illegible] (LLL, along with the Los Alamos [illegible], is the chief U.S. center for [illegible] development. Although [illegible] University of California [illegible] laboratories conduct their weapons [illegible] of university involvement. McDonald [illegible] join R&D Associates [illegible].)

Dominic A. Paolucci [illegible] Arlington, Va. [illegible] vate defense consulting [illegible] ganization has been [illegible] our mail inquiry to [illegible] captain, retired [illegible] with advanced [illegible] and military fields.)

Richard L. Wagner. Physicist, Lawrence [illegible] tory, Livermore, Ca. (Wagner is a [illegible] pons design and military applications [illegible] more Laboratory.)

Source: First Annual Report of the President to the Congress on Federal Advisory Committees, U.S. Senate Committee on Government Operations, May 2, page 1317; additional data (in parentheses) is from a variety of public sources.

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ment or computer program in the world. Several sets of considerations would naturally enter into the detailed creation of such plans:

1. Political. Do we have a counterforce or a countervalue policy? Apparently, according to Hersh's sources, we have had a mixture of the two for some time. Now, according to Schlesinger, we are moving more toward the counterforce policy.
2. Military. What is the priority list of targets within any given policy? What is the command and control structure to initiate and carry out nuclear operations?
3. Technical. Given the varied technical capabilities of the several types of nuclear weapons at our command, and the varied characteristics of the targets, what is the optimal assignment of missions to all components?

Of course, these three sets of considerations are not independent of one another; and one should not imagine that decisions always proceed in the simple sequence 1,2,3. Students of the arms race have observed that often technical analyses and innovations have come first, leading to concrete new weapons development, which then become de facto elements of national policy. One example of this is the story of MIRV, the multiple warhead for our nuclear missiles. (See Herbert York, "The Origins of MIRV," SIPRI Research Report No. 9 [Stockholm: Stockholm International Peace Research Institute, August 1973].)

MIRV is the most significant innovation in nuclear weaponry in recent years and, undoubtedly, it is very relevant to any discussion of SIOP. As York tells it, there were two missions for a multiple war-

head system that were prominent in Department of Defense discussions in the early and middle 1960s. One was a counterforce weapon to cover a growing number of Soviet military targets; the other was as a countervalue weapon to penetrate an anticipated Soviet ABM system which might shield their cities from a retaliatory attack. The military's decision to deploy MIRV (on land-based and on submarine-based missiles) was made in 1966, and actual installations began early in 1970. Thus, the simple existence of MIRV required that any previous targeting plan, namely SIOP, would have to be revised because there were now many additional warheads that had to be targeted somewhere. This need to redo SIOP could be merely a technical assignment within the given strategic policy or it could provide an opportunity for a qualitative change in strategy.

Another very important occurrence was the US.-USSR agreement in SALT I (finalized in May 1972) to limit ABM systems. This step wiped out one of the two original missions imagined for MIRV—its countervalue role in penetrating an ABM defense—and left the counterforce mission as the only option for this expanding number of available warheads.

The problem of assigning targets to the multiple warheads of a MIRVed missile system poses some complicated technical questions of control, coordination, optimization, and the like. This is where the Scientific Advisory Group would be expected to enter in an important way. Is this committee's work only technical, or might it be expected to have an influence on policy? The advisory committee is not expected to do the detailed work of writing the computer program for SIOP; its job is to oversee, to provide advice and guidance. While all the members of this advisory committee have technical training in their backgrounds, it is clear that at present most of them hold top level executive positions in their respective organizations and are accustomed to shaping policies as well as administering them.

The history of nuclear weapons development—from its very beginning—has been full of occasions where scientific advisors have had a crucial influence on the formation of military policy. The evidence we have collected showing the very hawkish composure of this committee, together with the retargeting opportunity posed by the MIRV system, leads us to believe that the work of the Scientific Advisory Group for the Joint Strategic Target Planning Staff was the nexus of the decision to embark on the new counterforce strategy.

Earlier, we posed the question of why Schlesinger decided to push a public campaign to move U.S. nuclear forces more toward the counterforce program. He is not reported to be a particularly hawkish person, nor is it convincing that he has taken up this campaign simply as a way to promote the bureaucratic interests of his department: the budgetary effects of this policy change do not appear to be very significant. On the basis of the analysis up to this point, we are now led to make a guess: that the change of SIOP toward a stronger counterforce strategy was carried out under the self-generated authority of the Joint Chiefs of Staff and then presented to Schlesinger as a fait accompli.

Such an idea is probably unwelcome to the thinking of most Americans, used to the principle that our military establishment is subject to civilian rule. However, one should not overlook the paralysis and instability that infected the Presidency during Watergate. There were three different Secretaries of Defense in the first half of 1973, and the opportunity for military leaders to act on their own was there. We will not suggest that the hawkish scientific advisory group on SIOP was the sole author of a secret shift in basic nuclear policy; surely a number of officers within the Department of Defense had to participate. We will propose that this scientific advisory group constituted the outside collaborators in a subtle but significant military *coup*.

Some readers may feel that this author is being unduly imaginative and alarmist in the conclusions of the last few paragraphs. It is legitimate to consider how different sets of readers, seeing the facts collected here and having their own inclinations, may be motivated to respond. In particular, consider a Soviet military analyst. Such a person would have every good reason to consider the most sinister suggestions seriously. The move of the United States toward more of a counterforce posture may signal the possibility of the United States achieving a first-strike capability, and the Soviets would be motivated to respond in kind. It is in such action-reaction cycles that we fear the greatest likelihood of upsetting the nuclear balance and precipitating a disastrous nuclear war. □

Department of Energy
Washington, D.C. 20545

MAY 17 1979

Mrs. Ruth Clusen
Assistant Secretary for Environment
Department of Energy
Washington, D.C. 20545

Dear Mrs. Clusen:

Consistent with notice of hearing issued on Friday, March 2, 1979 (43 F.R. 11821) public hearings were held on the Draft Environmental Impact Statement, Livermore Site, DOE/EIS-0028-D, on April 12, 1979. The hearings were conducted by the undersigned Presiding Board in conformance with the procedures set forth in the notice.

Following review of the Draft Environmental Impact Statement and of the record compiled, the Board has identified some issues which may be critical to future decision making. The attached report sets forth these issues as well as the Board's recommendation concerning their treatment in the final impact statement.

In addition, the Board has compiled a record of the hearings, consisting of the transcripts, and the written statements, documents and exhibits submitted by private persons and organizations, including the written comments submitted after the close of the hearings in response to Board request. In accordance with the notice, this hearing record and the Board's report is being transmitted to the Department's public document rooms.

Further, in accordance with the notice of hearing and the mandate to this Board, this report is limited to those unresolved issues relating to DOE/EIS-0028-D which the Board determined critical to future decision making regarding

the Livermore Site. This Board has neither undertaken to resolve the issues raised nor to render judgements concerning such issues. We trust, however, that their identification and our recommendations on how that should be addressed will assist the decision making process.

Respectfully,

THE PRESIDING BOARD

G. Victor Beard

G. Victor Beard

L. Trowbridge Grose

L. Trowbridge Grose

John B. Farmakides

John B. Farmakides, Chairman

Enclosure:
As stated

STATEMENT
IDENTIFYING VIEWS AND ISSUES ON
THE DRAFT ENVIRONMENTAL IMPACT STATEMENT
LIVERMORE SITE, DOE/EIS-0028-D
by the
PRESIDING BOARD

May 14, 1979

I. INTRODUCTION

In accordance with the notice of hearing dated March 2, 1979, the public hearings on the Draft Environmental Impact Statement, Livermore Site, (DOE/EIS-0028-D) were held on April 12, 1979 in Livermore, California. The hearings were conducted by the Presiding Board (Board) established for this proceeding under the rules of procedure set forth in the notice.

Comments were received from private and public organizations and from individual citizens.[1/] As stated in the notice of hearing, the Board has identified only those issues which it considers to be critical to future decisionmaking regarding the Livermore Site. In addition, and as noted by the Board during the course of the hearing, the Presiding Board has not undertaken to judge the merits of the issues or to render judgment concerning the course of the operations. This report, along with copies of the transcript, oral and written comments

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1/ Many of the participants raised similar or identical issues. Where possible, those issues that would so permit have been consolidated. In addition, the Board has not attempted to reference in this report all the participants who addressed a specific issue. That information is available in the transcript and in the exhibits made of record.

and questions submitted to the Board by members of the public, organizations and government agencies, as well as the list of exhibits[2/] attached as Appendix A hereto, including the written responses by the staff to the questions raised in the hearing, constitute the record of this public hearing. Concurrent with the submission of this report to the Assistant Secretary for Environment, it is being sent to the Acting Deputy Director, Office of Environmental Compliance and Overview, for placement in the Department's public document rooms.

II. UNRESOLVED ISSUES

A. Geological-Seismological

The key issue most frequently identified by participants, including both technical experts and laymen involves the earthquake (seismic) safety of continued operations of the Laboratories on the Livermore Site.

The potential for earthquakes and the effects of earthquakes relative to the Livermore Site is obviously a complex subject. It continues to receive a great deal of attention (as many other areas do in California) by the LLL staff, by the USGS, by academia, and by industrial and utility companies. The numerous comments received range all the way from: 1) the site must be abandoned because there is no

2/ Three participants were afforded the opportunity to submit their comments and questions to the staff for further clarification provided the same was done by April 16, 1979. (Hearing Transcript (Tr.) pp. 46, 98, 103). Only one such letter was received: recorded as Exhibit #13.

protection possible from inevitable earthquakes to 2) the site is well suited to safely withstand the most damaging earthquake that may strike. The staff is fully aware of the importance of this issue and has described in some detail their plans for future investigations to resolve remaining questions on seismic safety.[3/]

Specific subtopics of this general seismic issue were repeatedly raised by many respondents in writing and at the public hearings. They are both interrelated and interdependent. The Board believes that some of them are of critical importance to future decisionmaking and require further attention. These subtopics are briefly discussed as follows:

1. Determination of the safe shutdown earthquake (SSE) or seismic hazard. Considerable concern was expressed about different stated values of acceleration, mainly 0.5g as compared to 0.8.[4/] Various investigations for LLL over the past several years have incorporated different criteria for determination of levels of conservatism used to arrive at "g" values. New data, (characterization of the Las Positas fault, renewed study of the Tesla faults and increasing deployment of seismicity monitoring networks, etc) should improve ability to arrive at a firm and convincing "g" value

3/ Staff Statement in response to comments received on the DEIS is dated March 1979 (Hereafter: Staff Stat.)., p 8-17; Tr. 192-198, J. Scheimer. See also Summary Statement by J. La Grone, Tr. 12-15.

4/ DEIS pp. 2A-29-30; staff state, p. 14-16; Tr. 198-200, D. Bernreuter; Tr. 201-202, J. Scheimer; Tr. Supp. 8-10, G. Barlow; Tr. 152, H. Rothblatt; Tr. 162, A. Baldwin; Ltr. from J. N. Brune to J. Farmakides, April 6, 1979; Tr. 27, J. Olsen, Tr. p. 223-4, F. Tocars.

based on most up-to-date state-of-the-art methods applicable to the Livermore site. Accordingly, clarification of the methods used to determine the SSE and "g" value and why these values differ from those of the Vallecitos site is needed in the FEIS.

2. Relative damage effects of possible instantaneous very high peak accelerations compared against a longer duration of strong ground motion. With more instrumentation of earthquake activity from various areas in the world, it is becoming apparent that unexpectedly high acceleration can occur from moderate magnitude earthquakes in areas close to the causative fault.[5/] Also duration of ground shaking can vary through seconds to tens of seconds.[6/] As a matter of earthquake engineering for safe seismic design criteria of critical facilities, some discussion is needed in the FEIS directed to the relative importance of damage potential from peak acceleration as well as from longer duration ground motion.[7/] Also needed is a discussion of the measures to be taken by the laboratories to minimize these seismic effects.

3. Re-evaluation of geology of Livermore Valley with emphasis on delineation of faults. Considerable concern was

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5/ Ltr. from J. N. Brune; Tr. p. 213-215, G. Barlow.

6/ Tr. p. 217, G. Barlow; Ltr. from P.M. Griffin to J. Farmakides.

7/ Tr. 206-211, R. Gray; Tr. 180-183, J. Caid; Tr. 183-187, J. Autherford. Consideration should be given to treating such complex technical issues as may require detailed explanations in appendices to the main text. This would maintain the desired brevity of the main text while providing detail for those having interest in the specific subject involved.

expressed by participants on the occurrence of faults bordering the Valley, within the Valley, and especially near and under the Livermore Site.[8/] Various maps and cross sections in the DEIS,[9/] while they necessarily differ in scale, date of preparation, data base available, purpose, and theme, collectively reveal a confusing, if not conflicting, array of faults. Up-to-date clear maps and cross sections are necessary to resolve questions of known and inferred faults, their extent, sense of displacement, geometry, possible interrelationships, potential for seismic activity, and very importantly, their ages.

As an example of the confusion that exists, the recently postulated "Las Positas fault" should, if possible, be verified and characterized. And while the delineation of local faults may have no affect on the determination of the SSE from the Tesla fault, the potential for surface rupture and local seismicity generated on nearby faults should be considered. The Tesla fault no. 1 in particular is mapped

8/ DEIS p. 2A-6-2A-21, 2C-10-2C-19; Staff Stat. 8-14; Tr. p. 21, C. Bowen; Tr. Supp. 6-8, 11, G. Barlow; Tr. 104-105, W. Riggin; Tr. 162-163, A. Baldwin; Tr. 166-168, R. Stolzman; Tr. 219-220, G. Barlow; Tr. 232-235, R. Zatkin; P.M. Griffin ltr.; Ltr. from Friends of the Earth to W. H. Pennington dated Dec. 18, 1978; Ltr. from W. H. Fraley to C. Jackson dated April 2, 1979; Ltr. from W. Riggan and S. Ladd to W. H. Pennington dated Dec. 18, 1978; Ltr. from L. A. Arnold to W. H. Pennington dated Dec. 17, 1978; Ltr. from L. E. Meierotto to W. H. Pennington dated Dec. 8, 1978.

9/ DEIS p. 2A-16, 2A-17, 2A-18, 2C-2, 2C-9, 2C-11, 2C-15, 2C-16, 2C-17.

as inferred to end at a point about midway along the southern boundary of the Livermore Site. The Tesla fault no. 2 is inferred to pass either just outside the southwest corner of the site[10/] or inside the site.[11/] Though the uncertainty of fault occurrence is probably real, it is compounded by conflicting map data. The staff noted its intention to obtain additional data and to undertake study of these and other nearby faults, their age(s), and their potential for surface rupture.

Another aspect of the geology of the Livermore Site is the definition of bedrock, the definition of soil, and the thickness and structure of each beneath the Site and in Livermore Valley in general.[12/] These concerns under the heading of subsurface geology, appear to need further amplification and reassessment. For example, depth to Bedrock was questioned as well as soil layers stated to be 1000 feet thick.

An improved seismotectonic model of the Livermore Valley and a new understanding of the relationship of local faults to regional faults should contribute to an improved estimate of the maximum credible earthquake (SSE) and surface rupture potential. It was pointed out[13/] that the Tesla fault and

10/ DEIS p. 2A-17.
11/ DEIS p. 2C-9.
12/ DEIS p. 2A-23-2A-26; Ltr. from P.M. Griffin; Ltr. from R. Zatkin; Tr. p. 216-218, G. Barlow; Tr. p. 180-181, J. Caid.
13/ Ltr. from Friends of the Earth; Ltr. from L. E. Meierotto, Ltr. from W. Riggan and S. Ladd; Ltr. from R. Zatkin; Tr. 165-172, R. Stolzman.

the Las Positas fault may be a significant strand of much longer and possibly active fault systems, hence influencing the determination of earthquake magnitude based on fault length and displacement relationships. The staff states that in light of new data generated from their studies of the Vallecitos site and as a result of the "discovery" of the Las Positas fault, they plan to update their seismotectonic model for the Livermore region.14/

4. Surface rupture potential. Although the concern about actual surface rupture potential within the Livermore Site is discussed above, this issue is emphasized because it was raised repeatedly and in detail by both laymen and experts. It would appear that evaluation of this issue requires a reasonable determination of the capability of faults running through the site. (Tesla fault nos. 1, 2 and 3; Ramp thrust; Corral Hollow; Doutherty; and Las Positas)

5. Integrity of existing facilities in face of possible seismic shaking and of surface rupture was a subject of considerable concern.15/ The subject of safe seismic design is extremely complex and is one in which relatively little experience and testing are available. A better understanding of the Site geology, and further evaluation of the SSE and the Operating Basis Earthquake (OBE) should contribute to

14/ Staff Stat. p. 9-12.

15/ Tr. 183-187, J. Rutherford; Tr. 180-183, J. Caid; Tr. 204-211, R. Gray.

clarify some important concerns raised on this subject. The Board recommends that this subject be examined thoroughly in light of new information being generated in the field of earthquake engineering.

6. The subject of seismic and geologic sitting criteria applicable to the Livermore Site. In view of the questions raised on this issue and the response of the staff it appears that further discussion and clarification of seismic safety standards for the Livermore Site, or for its more critical facilities, is needed in the FEIS.16/

7. Several participants urged the need for an independent review and critique of any forthcoming reports on seismic hazard assessments at the Livermore Site.17/ They were of the opinion that independent review is necessary to provide impartial credibility to such reports.

B. Health Effects

1. Radiological Standards. The use of the current standards established for DOE laboratories including those at Livermore was questioned by a number of participants.18/ These questions were centered around (a) low level-long time exposure and (b) plutonium-lung and bone-exposures.

These participants proposed that epidemological methods used by "Mancuso", "Sternglass", "Lyon", and others

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16/ Staff Stat. p. 14-15; Tr. 198-99, D. Bernreuter, Tr. 182, J. Caid; Tr. Supp. p. 7-10, G. Barlow; Tr. p. 313, J. B. Dickenson.

17/ Tr. 106, W. Riggan; Tr. 278-279, G. Oilley; Tr. 303, J. D. Smith; Tr. 314, J. B. Dickenson.

18/ Tr. 22-29, C. Bowen; Tr. 79-99, C. Johnson; Tr. 34-55, S. Ladd; Tr. 106, W. Riggan; Tr. 118-119, C. Schwartz; Tr. 164, 225, A. Baldwin, etc.

be used for long time-low dose "exposures". It was also suggested that plutonium standards proposed by "Gofman", "Burleigh" and some Russian studies be adopted for plutonium lung and bone exposures.

The staff responded that while they were aware of the current literature concerning this question, they had adopted the standards set forth in Title 10 CFR, Part 20 and related pertinent portions. These standards are endorsed by the International Commission on Radiological Protection and the National Council on Radiation, Protection and Measurements.[19] The staff also noted that there are uncertainties implicit in epidemiological methods, measurements and calculations at low dose and long term exposures. There are also uncertainties involved in straight line extrapolations of gross biological effects against dose at low doses.

It would appear necessary, in view of the concerns voiced in this area that the FEIS address this subject in more detail to include a summary of the efforts currently in process towards the review and evaluation of such standards.

2. Compliance with Radiological Standards. There were several comments criticizing the laboratory and field methods used to determine compliance with the radiological standards.[20] One participant, Dr. Carl Johnson, stressed his

19/ See DEIS, pp. 3-74, items 3-7, 3-8.

20/ Tr. 29, C. Bower; Tr. 81-90, 93-97, C. Johnson; Tr. 151-152, H. Rothblatt.

concern with the methods for plutonium sampling in soil.[21/] In its response, the staff noted that Dr. Johnson's methods were not universally recognized, and in fact, were in conflict with the standards of both the Environmental Protection Agency and the Nuclear Regulatory Commission.[22/] However, it would appear from both written and oral comments and from the published literature available that soil sampling for respirable particles is in a state of flux and that this discipline should be constantly monitored for improved acceptable methods.[23/]

From the record available, it would seem that the Livermore laboratories are complying with the adopted standards.[24/] This subject should be further discussed in the FEIS.

3. Toxicological Standards. Concern was raised as to the amounts of toxicological material released to the environment, and methods used to ensure compliance with adopted standards.[25/] The labs have agreed to enlarge some sections of the FEIS giving more information in certain critical areas. We believe this to be desirable; however, it should not be overdone. Much of the detailed information asked by

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21/ Tr. 82-97, C. Johnson.
22/ Staff Stat. pp. 6, 7.
23/ See also DEIS pp. 3-74, references: 3-1, 3-2, 3-3, 3-4, 3-13, 3-14.
24/ DEIS pp. 2-28, 2-44, 2-45, 2-60, 2-63, 2-64, 2-67, 3-22, 3-16, 3-17. See particularly Staff Stat. pp. 30 which shows the whole body exposure of Livermore employees in 1978 to be extremely low.
25/ Ltr. W. Talley to H. Pennington dated Apr. 13, 1979; Tr. 80, C. Johnson.

some individuals can best be furnished by the existing lab reports. To include this vast amount of material in the report would detract from its desirable compact size.

C. Maximum Credible Accident (MCA)

Concern was repeatedly expressed[26/] that the DEIS was inadequate in the definition and evaluation of the MCA. Since the MCA may be determined by the Safe Shutdown Earthquake (SSE) this subject should be discussed in sufficient detail to permit meaningful evaluation. The FEIS should be clarified as to this issue, including a discussion as to the plans developed for responding to an MCA.

The major concern of many participants centered around possible earthquake rupture of the two buildings containing plutonium and the fate of the plutonium if it were to escape its containment environment. Apparently, the Livermore laboratory uses a multiple containment philosophy which depends in large part in maintaining negative air pressure between certain pieces of equipment (glove boxes) and the room in which they are contained, as well as maintaining negative pressures between this room and other parts of the building. Obviously this philosophy depends on maintaining the integrity of the ducting system, the pressure pumps, and the motors driving the pumps. Many of the participants opined that the DEIS was inadequate because of its failure to discuss this

26/ Tr. 205, R. Gray; Tr. 281, G. Dilley, Tr. 284, D. Page; Tr. 309, J. Smith; Tr. 313, J. Dickenson.

system in detail, especially as it may be affected by an earthquake. For example, in case the containment building housing plutonium was breached by such an earthquake, what would happen to the available plutonium if fire develops and it becomes airborne.

It would appear that a short appendix containing surface-to-volume relations and how they affect burning rates, slagging, and particle size distribution of the metal oxide should be added to the FEIS; otherwise, an attempt should be made to relate the maximum credible accident concerning plutonium to other actual experiences, assuming the containment is breached.

It should be noted also that LLL and SLL handle accidental releases of titium with substantially different systems. Unnecessary speculation is promoted because the DEIS is unclear as to the reasons for this difference. This question should be more fully discussed in the FEIS.

D. Socio-Economic and Other Issues

1. One group of participants[27/] state that the DEIS is defective because it does not adequately analyze and evaluate the "benefits" of the Livermore Site. The discussion in the DEIS on the benefits of the Livermore site is considered, in their opinion, to be a mere conclusion, i.e. that the nuclear weapons policy of the United States is necessarily beneficial.

27/ For example, see Tr. 57, D. Elsberg; Tr. 108, C. Schwartz; Tr. 147, W. Reynolds; Tr. 260, M. Olney; Tr. 33, S. Ladd, etc.

They maintain that such a conclusion is insufficient to properly evaluate, assess, and balance the environmental costs involved. For example, one witness contended that the assumptions for continued operation of the Livermore Labs based on the needs of the national nuclear weapons program are incorrect; that in view of the status of nuclear technology and the inventory of nuclear weapons, each of which far outweigh the need, the benefits of continued operation of the Livermore site could not be shown.[28/] He was of the opinion that the Livermore DEIS, can be meaningful only within the context of an environmental impact analysis of the entire national nuclear weapons program; which analysis could conceivably result in a conclusion to stop further nuclear weapons design and testing and thus reduce or eliminate the need for the Lawrence Livermore Labs.

One witness commented that nuclear weapons could "never be made ethically acceptable in terms of public health and genetic integrity", and, therefore should be abandoned. However, he favored continued existence of the research and development effort at DOE and the Livermore laboratories so long as there exists a need to "stay-even-with, or ahead of /a potential/ enemy.[29/]

In its response the staff notes that the DEIS is a site specific encironmental impact statement and fully

28/ Tr. 74-77, D. Elsberg.
29/ "Comments on the Environmental Impact Statement for the Lawrence Livermore Laboratory", Dr. John Gofman, p. 2-3, Tr. 160.

meets the requirements of NEPA. They further note that the national policy with respect to nuclear weapons design and production is imposed by both the Congress and the President and that the Department has no role as to the deployment and use of such weapons systems.[30/]

While the staff response to this issue may be technically correct, nevertheless, the Board believes that the issue should be brought to the attention of the appropriate decisionmakers.

2. Another issue relates to the analysis of the "economic" cost benefit of the continued operation of the Livermore laboratories. Several participants[31/] noted that the DEIS did not adequately address the various economic factors that are involved. For example, concern was[32/] expressed that the economic costs of alternatives were not adequately considered since the ripple effects of the appropriations spent at the Livermore Laboratories, considered to be synonomous to military spending, are purportedly not as advantageous economically as if spent for commercial or industrial products. The witness also testified that the high probability of test ban treaty and its impact on the Livermore Labs should also be discussed -- including the

30/ Staff Stat. pp. 2-3.
31/ Ltr. F.O.E. to H. Pennington, p. 5; Tr. 245-250, D. Thomas-glass; Tr. 302-304, J. Smith. But see Tr. 176-177, D. Hughes; Tr. 178-180, H. Hubbard.
32/ Tr. 248-252, D. Thomas-glass; Tr. 259, A. Barreiro.

impact such a treaty may have in reducing substantially the number of employees at Livermore and thus directly affecting the cost-benefit balance. Several suggestions were made that in performing this type of analysis, the alternative of assessing the use of Livermore laboratories for study and research on all energy options and technologies should be more adequately considered.

E. Emergency Response Plan.

Concern was voiced[33/] that the emergency response and evacuation systems in the Livermore area are inadequate and not properly evaluated in the DEIS as to any off-site evacuation that may be made necessary by reason of the continued operations of the Livermore site. For example, apparently there is no suitable human radiological decontamination facility available in the region and the relevance of this fact to the continued operations of the laboratories was not discussed in the DEIS. In response the staff noted that while the laboratories were ready to assist as much as possible, such emergency plans were the responsibility of the local governments.[34/] The extent of the responsibility of the laboratories and the resources available in the event of an MCE should be treated in the FEIS. In addition, in view of the expressed concerns, it would appear that the laboratories may need to become more active in their attempts

33/ Tr. 269-277, V. Miller; Tr. 283, 284, D. Page; Tr. 20, 21, C. Bauer; Tr. 315, J. Dickenson; Tr. 317, D. Ellis.
34/ See Staff Stat. Supplement, p. 28, DEIS Appendix 3.

to assist the local and state agencies in developing such plans.

F. Miscellaneous

1. The transportation of radioactive materials into and out of the Livermore site was identified as a hazard that has not been adequately addressed in the DEIS.35/ For example, one witness noted that the containers used for shipping radioactive materials were inadequate and should be more fully discussed in relation to the type, and level, of radioactive materials being transported.36/ On the other hand, the staff points out that the containers used for the transport of radioactive materials (non-weapon related) are built to the specifications and standards of the U.S. Department of Transportation.37/ One available option is to obtain clarification from the Department of Transportation on this question for later inclusion in the FEIS.

2. The DEIS was also criticized by some38/ because it did not discuss various alternative options for the use of the Livermore site to the extent they thought necessary. They were of the opinion that the alternative uses for the site for all energy research should be more fully evaluated to include conversion of the facilities to solar energy research, etc. This issue should be clarified in the FEIS.

35/ Tr. 139-147, W. Reynolds; Tr. 86, C. Johnson.
36/ Tr. 143-145, W. Reynolds.
37/ DEIS 3-9, 2, 7; see also Staff Statement dated March 1979, p. 32.
38/ Tr. 38, S. Ladd; Tr. 245, Thomas-glass; Tr. 278, D. Dilley.

3. One participant requested names of the authors to the various sections of the DEIS be identified so as to enhance the credibility of the DEIS and to complete the record of the proceeding.[39/] The staff response was that it is difficult to pin point the author of a specific section in view of the many people involved, because of the redrafting which occurs, and because the report is a total team project.[40/] One suggestion advanced is that detailed scientific and technical material could be presented in appendices, with authorship of those appendices available to the public on specific request provided that it does not impinge on the objectivity of the individual authors, or does not result in their harrassment.

4. In conjunction with the statement by the staff that the Livermore Labs have undertaken a detailed plan to investigate the seismic activity of the entire area[41/] a request was made that a public hearing be convened at a later time after the seismological data is compiled. The Board noted that it has no authority to convene such a hearing, however, in view of the request as it related to the seismic issues it agreed to raise the question for resolution by the appropriate decisionmaker.

39/ Tr. 242, 243, R. Zatkin.
40/ Tr. 242, 243, R. Duval. It is appropriate that the Board note for the record the cooperation and responsibility displayed by the staff during the hearing in attempting to answer as many questions as possible, and to address the issues raised.

41/ Tr. 193-197, J. Scheimer.

APPENDIX A

DOE/EIS-0028-D
Public Hearings
Livermore Site
April 12, 1979

Exhibits and Written Statements Submitted at Hearings:

Ex. #1	Ltr. fr. Valerie A. Raymond, Chm. Bd. of Spvs., 1st Dist. Cty. of Alameda,CA., to C. Jackson, DOE., requesting that several reports along w/ltr. be in record.	Tr. 15
#2	Statement fr. Congr. R.V.Dellums, 8th Dist., CA., for DOE Hearing dated April 12, 1979	Tr. 19
#3	Mr. D. Ellsberg adopted statement of Natural Resources Defense Council, Inc., 12/21/77 on Rocky Flats Plant Site, DEIS ERDA-1545-D, and submitted it as his exhibit.	Tr. 78
#4	Exhibit by Prof. C.Schwartz, including series of letters and articles plus copies of 2 posters prepared by Prof. Schwartz.	Tr. 108
#5	Series of documents by Dr. J. W. Gofman, M.D., Ph.D., addressing the cancer hazard from inhaled plutonium; Testimony for the GESMO Hearings	Tr. 160
#6	Exhibits submitted by Friends of The Earth:	
	(a) Letter dated 4/10/79 to J. B. Farmakides	
	(b) Ltr. J. N. Brune, Prof.Geophysics, U.Cal. at San Diego, dated 4/6/79 to J.B.Farmakides, enclosing testimony on behalf of Intervenors Regarding CONTENTION 3 - GROUND MOTION, DIABLO CANYON NUCLEAR POWER PLANT, UNITS 1 & 2 Docket No.s STN 50-275, 50-323	Tr. 212
	(c) Ltr. fr. P. M. Griffin dated 4/6/79 to J.Farmakides	Tr. 215

Ex. #6 (cont'd)

(d)	"Preliminary Analysis of the Peakes of Strong Earthquake Ground Motion - Dependence of Peaks on Earthquake Magnitude, Epicentral Distance and Recording Site Conditions" by Dr. M. D. Trifunac	Tr. 220
#7	Criticism Pertinent to: Appendix 2A Geological and Seismological Investigation of LLL Site by L. H. Wright of 6/3/74 submitted by Robert S. Zatkin	Tr. 230
#8	Exhibits submitted by Mr. J. Ventresca	Tr. 266
#9	Ltr. to Board by Mr. A.L. Barreiro, UC Santa Cruz, representing himself and others (1100 on campus)	Tr. 255
#10	Testimony presented by Grace Dilley, Berkeley, Calif., dated 4/4/79 addressed to W. H. Pennington, DOE	Tr. 278
#11	Response to DOE Staff Statement on Comments Received on DEIS for Livermore Site, March 1979, submitted by W. A. Lochstet, Environmental Coalition on Nuclear Power, by ltr. 4/9/79	
#12	Ltr. to J. Farmakides from Ms. D. Headley dated 4/14/79	
#13	Ltr. to R.A.DuVall dated 4/16/79 fr. W. Riggan,UC Nuclear Weapons Labs Conversion Project, submitting list of unanswered questions by staff - per his testimony at hearing	Tr. 103

COUNTY OF ALAMEDA
CALIFORNIA

BOARD OF SUPERVISORS

VALERIE A. RAYMOND
SUPERVISOR, FIRST DISTRICT

April 11, 1979

Mr. Calvin Jackson
United States Department of Energy
1333 Broadway
Oakland, CA 94612

Re: Draft Environmental Impact Statement (DEIS)
Lawrence Livermore Laboratory

Dear Mr. Jackson:

The Alameda County Board of Supervisors wishes to comment on the Draft Environmental Impact Statement prepared by the Department of Energy for the Lawrence Livermore Laboratory and have these comments entered into the record of the April 12, 1979 public hearing on this matter.

The Board believes that the present draft contains insufficient information, as outlined in the attached reports from the Alameda County Planning Commission, the Environmental Protection Agency, and County staff, and requests that these comments be considered and responded to in the Final Environmental Impact Statement.

Thank you for your consideration.

Sincerely,

Valerie Raymond

Valerie A. Raymond
Chairman

VAR:my
Encls.

1221 OAK STREET · SUITE 536 · OAKLAND, CALIFORNIA 94612 · (415) 874-7367

BOARD OF SUPERVISORS

VALERIE A. RAYMOND
SUPERVISOR, FIRST DISTRICT

COMMENTS ON LAWRENCE LIVERMORE LABORATORY/
DRAFT ENVIRONMENTAL IMPACT STATEMENT (DEIS)

The following comments are submitted by the Alameda County Board of Supervisors on the Draft Environmental Impact Statement (DEIS), prepared by the Department of Energy on the Lawrence Livermore Laboratory:

1. Data presentation and coverage.

   There should be a far more complete presentation of data and the final impact statement should be modified to address the following concerns:

   a) Summary concludes project operations pose no impact on surface or groundwater. No facts in report to support this conclusion.

   b) The tone of the report is very one-sided in that it emphasizes project benefits and minimizes potential hazards.

   c) No indication of amount of radioactive material stored and used on site is provided.

   d) The assumption is made that if radioactive material releases are below DOE standards then there are no impacts. With the current controversy over long-term effects of low level radiation and the setting of standards this assumption is difficult to support.

   e) The disposition of high-level radioactive waste is dismissed by stating that it is disposed at a DOE approved off-site area? Where is it? What are the impacts on Alameda County and the Bay Area?

   f) Safety procedures for handling liquid waste are described in detail and the impression is given that an accident is not possible. Accidents are always possible and, therefore, potential impacts on surface water and groundwater resources from an accidental release of contaminated liquid waste should be assessed.

   g) Impacts of accidental release of radioactive materials into the municipal sewage treatment plant should be assessed.

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2. Seismic Studies.

Additional seismic studies to be conducted be undertaken by independent seismic engineers or the U. S. Geological Survey to include:

a) A re-evaluation of data from: Herd's (1977) investigation of the Las Positas, Greenville, and Verona faults; J. A. Blume and Associates (1972, 1978) seismic and geologic investigations of the effects of the Tesla and Las Positas fault zones on the Lawrence Livermore and Sandia Livermore Laboratories; and the Vallecitos site studies. This will be used to develop a new tectonic model for the Livermore Valley.

b) The use of this new model to establish a field investigation with goals to resolve questions regarding the potential for surface faulting at the LLL/SLL sites and to develop reoccurrence intervals for active faults in the vicinity of the LLL/SLL sites; and

c) To perform a probabilistic based seismic hazard analysis for the Livermore site using appropriate activity levels as deducted from the field investigations.

The above program should be designed to evaluate potentially active fault zones that may lie under or near critical facilities sites and to address in detail seismic shaking hazard potentials that may have impact on the sites.

3. Emergency response plans.

The DEIS should include requirements for evacuation plans based on several possibilities from worst possible case to most probable case.

HAF:EHM:mso

ALAMEDA COUNTY PLANNING DEPARTMENT

399 Elmhurst Street, Hayward, California 94544 (415) 881-6401

April 2, 1979

Mr. Calvin Jackson
United States Department of Energy
1333 Broadway
Oakland, CA 94612

Dear Mr. Jackson:

The Alameda County Planning Commission has the responsibility of planning for lands in Alameda County which may be impacted by the operation of the Lawrence Livermore Laboratory. The Commission considered a staff report on the Draft Environmental Impact Statement Livermore Site, Livermore, California, September, 1978, prepared by the U.S. Department of Energy at their meeting of Monday, March 19, 1979 to become familiar with potential impacts.

The comments which follow are submitted to you for review and response in the Final Environmental Impact Statement.

1. The distribution of the DEIS to this Commission did not occur with sufficient time available to respond under the original schedule. This did not permit the time necessary to review the total document fully. The Commission had only one copy of the DEIS for its use.

2. The summary contained in the DEIS is brief to the point of being inadequate, and seemed to treat the substance of the report rather superficially.

3. There appears to be minimal correlation of the County General Plan and its elements with the evaluation made in the DEIS. It is questionable whether the County planning elements were reviewed as part of the preparation of the base document.

4. The DEIS appears deficient in its consideration of the impacts on the water resources in the Valley to the extent that the DEIS is judged to be inadequate for use in assessing the impacts the Lab may have on the surrounding community. This is particularly evident in the apparent lack of attention given to the impacts of any accident on the surface and ground water resources. The volume of liquid containing radio nuclides located at the laboratories is not identified nor is the direction of accidential spill addressed. In addition, air borne contamination that may be deposited on the surrounding properties with subsequent surface water runoff directly affecting the health and safety of the public and ultimately carrying the material back into the surface and underground water system has not been addressed.

5. The impacts of a potential nuclear contamination entering the municipal waste disposal system should be assessed.

Enclosure 2

6. There is lack of agreement by geological authorities regarding the location and potential effects of the Las Positas fault. Additional geologic studies should be made and included in the final DEIS to resolve all questions on this matter.

It is requested that the additional studies suggested above to bring the report into conformance with the Federal Guidelines for the preparation of a DEIS be completed and that the new information be distributed to those receiving this DEIS so that comments on the new information may be included in the final DEIS to be certified by the Department of Energy.

Thank you for your assistance and attention.

Very truly yours,

William H. Fraley
Secretary

WHF:rr

cc: Board of Supervisors
Director of Public Works

UNITED STATES ENVIRONMENTAL PROTECTION AGENCY
WASHINGTON, D.C. 20460

12 JAN 1979

Mr. W. H. Pennington
Mail Station E-201
GTN
Department of Energy
Washington, D.C. 20545

Dear Mr. Pennington:

Enclosed are the EPA review comments on the Draft Environmental Impact Statement, DOE/EIS-0028-D, entitled, "Livermore Site, Livermore, California".

Our major concerns with this environmental impact statement (EIS) are the lack of environmental data, the question of whether certain effluents are as low as reasonally achieveable (ALARA) and the incomplete dose assessment presented. There are several areas in the EIS where the data presented is not sufficient to allow independent analyses of the radiation impact of the facility. EPA believes that this data should be incorporated into the final statement. The question of achieving ALARA levels for effluents from several specific facilities are detailed in the enclosed comments. Finally, the failure to present population doses, health effects estimates, food and water pathways considerations, radionuclides considered, and the methods and assumptions employed result in an unacceptably incomplete picture of the laboratory's environmental impact.

In light of our review and in accordance with EPA procedures, we have rated the proposed action LO (Lack of Objectives) and classified the statement as Category 2 (Insufficient Information). If you or your staff have any questions concerning our rating or comments, please do not hesitate to call on us.

Sincerely yours,

for William D. Dickerson

Peter Cook
Acting Director
Office of Federal Activities (A-104)

Enclosure

EPA COMMENTS ON DOE/EIS-0028-D
THE DRAFT ENVIRONMENTAL IMPACT STATEMENT
ON THE LAWRENCE LIVERMORE LABORATORY SITE AT
LIVERMORE, CALIFORNIA

General Comments

1. This draft environmental impact statement (DEIS) relies heavily on referencing other reports, probably in order to minimize the size of the document. However, in some cases there is not enough information to enable a reviewer who does not have access to the references to determine the effect of site operations on the environment. The most significant omissions are in describing the environmental monitoring program and in summarizing data collected over the years to show whether any trend is apparent. There is also a need for better maps of both the site and the surrounding area. More detail is included in specific comments below. The final EIS (FEIS) should address these items.

2. EPA understands that the description of current and proposed activities is not up to date and that current plans are different in some cases. The FEIS should be current in all significant ongoing and proposed activities.

3. More information and discussion are needed to assure that the radioactive effluents that may affect persons off-site are at a level considered to be as low as reasonably achieveable (ALARA). The most significant of these are:

(a) The 14 Mev neutron generator which is projected to deliver a fence line dose of 900 mrem per year. This is a high level of radiation for an unrestricted area and there is a need to more explicitly discuss whether this level is ALARA. Also needing discussion are when (and if) the relocation to Building 292 will occur and what the expected fence-line doses from the new facility will be;

(b) The procedure which is relied upon for the Livermore sewage treatment plant to divert contaminated effluent at the plant rather than providing hold-up capability at the site;

(c) The reasons why tritium releases to air and water cannot be further reduced needs to be explained; and,

(d) The expected radiation exposure to passenger traffic from the LINAC, reactor, and relocated neutron generator operations if the new northwest entrance is still planned.

Enclosure 4

4. There is a deficiency within the DEIS in the description of the data produced by the off-site environmental sampling program. Although detailed information on the sampling locations, the types of samples (i.e., sewer sludge, water, air), and the radionuclide concentrations are provided in the annual reports, from all we believe the FEIS should include a summary of much of the information contained in these reports to allow a complete, independent assessment of the Lawrence Livermore Laboratory (LLL) impact on the local environment. It would be particularly helpful to have a summary of average annual radionuclide concentratons in effluents and in all media for the past five years to aid in relating the annual effluent releases to present radionuclide concentrations in the immediate environs. The use of several years' data would also reflect the apparent variability of site operations over time and would better indicate the full range of possible environmental effects than could be obtained by choosing a single year.

Further, all radionuclide releases from Sandia Laboratories - Livermore (SLL), including liquid tritum releases, should be included in the annual release data (Table 3-1).

5. More discussion is needed on the environmental effects of site operation as determined from summarized trend data. Two areas of special interest are groundwater contamination at both the Livermore site and Site 300 and the effects of discharge from the Livermore Water Reclamation Plant (LWRP).

The Livermore-Amador Valley Wastewater Management Authority Project (presently under construction) will result in LWRP effluent being transported out of the valley and discharged into San Francisco Bay. This will significantly change the fate of future LLL liquid discharges; it should be addressed in the FEIS.

6. EPA questions the procedure of comparing effects from accidents at LLL with the 10 CFR 100 regulatory limits for accidents, this was done in several places in the DEIS. 10 CFR 100 applies strictly to accidents at reactor sites. If the accident doses from various LLL sources are to be compared with it, there needs to be an explanation of how these regulations relate to the accidents being analyzed. On a related subject, it is stated that DOE guidance may be used to triple the levels provided in the EPA Protective Action Guides (PAGs) before evacuation needs to be considered. This is an inappropriate extrapolation of the PAGs. Currently the PAGs are only Agency guidance and provide action ranges for only the whole body (1-5 rem) and the thyroid (5-25 rem). There is no provision made for further extending these ranges. Further, protective action does not necessarily mean an action as drastic as evacuation. Protective action can be action that will reduce exposure or the chance of exposure.

Eisenhower's other warning

In his farewell address, President Eisenhower issued two warnings to the American people. The first of these is very well known, it fits easily into a variety of ideological frameworks and it is often quoted or paraphrased. We must, he said, be wary of "the acquisition of unwarranted influence, whether sought or unsought, by the military-industrial complex."

Eisenhower's second warning is much less well known, it is not so easily understood and it is seldom quoted except by specialists studying the Eisenhower administration. After noting that research played an increasingly crucial role in our society and that the ways in which it was conducted had changed radically in recent years, Eisenhower said, "Yet in holding scientific research and discovery in respect, as we should, we must also be alert to the equal and opposite danger that public policy could itself become the captive of a scientific-technological elite."

To understand this second warning, it is necessary to recall its context. This context consisted of the events that took place during the forty months from the launching of Sputnik to the end of his administration. The particular segment of the "scientific and technological elite" that he had in mind consisted of the hard-sell technologists who tried to exploit Sputnik and the missile-gap psychosis it engendered. We should be wary, he said, of accepting their claims, believing their analyses, and buying their wares. They and their sycophants invented the term "missile gap," they embellished that simple phrase with ornate horror stories about imminent threats to our very existence as a nation, and they offered a thousand and one technical delights for remedying the situation. Most of their proposals were expensive, most were complicated and baroque, and most were loaded with more engineering virtuosity than good sense. Anyone who did not immediately agree with their assessments of the situation and who failed to recognize the necessity of proceeding forthwith on the development and production of their solutions was said to be out of touch with reality, technically backward, and trying to put the budget ahead of survival.

The claims of such people that they could solve the problem if only someone would unleash them carried a lot of weight with the public and with large segments of the Congress and the press. Other

GEORGE TAMES, NYT PICTURES

DWIGHT D. EISENHOWER—1956

scientists and technologists had performed seeming miracles in the recent past, and it was not unnatural to suppose that they could do it again. It seemed that radar had saved Britain, that the A-bomb had ended the war, and that the H-bomb had come along in the nick of time to save us from the Russian A-bomb. On the home front, the relatively recent introduction of antibiotics had saved our children from the scourges of earlier times, and airplanes and electronics had become capable of carrying us, our words and our images great distances in short times. Scientists and technologists had acquired the reputation of being magicians who had access to a special source of information and wisdom out of reach of the rest of mankind. A large part of the public was therefore more than ready to accept the hard-sell technologist's view of the world and to urge that the government support him in the manner to which he wanted to become accustomed. It seemed as if the pursuit of expensive and complicated technology as an end in itself might very well become an accepted part of America's way of life.

But it was not only the general public that believed the technologists understood something the rest of the world could not. Many of the scientists and technologists themselves believed that only they understood the problem. As a consequence, many of them believed it was their patriotic duty to save the rest of us whether we wanted them to or not. They made their own analyses of what the Soviets had done. They used their own narrow way of viewing things to figure out what the Russians ought to do next. They then argued that since the Russians were rational (about these things anyway), what they ought to do next is what they must in fact now be doing, and they then determined to save us from the consequences of this next real or imaginary Russian technological threat. The Eisenhower Administration was able to deal successfully and sensibly with most of the resulting rush of wild ideas, phony intelligence, and hard sell. But some of these ideas did get through, at least for a while. Beyond that, dealing with self-righteous extremists who have all the answers—and there were many among the scientists and technologists at the time—is always annoying and irritating.

As we now know, the commonly baroque and occasionally bizarre technological ideas urged on us in those years were in fact a portent of things to come. Weapons systems and other high technology devices have become still more complex in the years since Eisenhower's farewell address. And this complexity is creating serious social and political problems of the general kind that Eisenhower warned us about. Today, there are even more people who tell us that because major weapons systems are so complicated only weapons experts can decide if they are needed, only those in on all the secrets and up on the most arcane elements of operations analysis can tell us whether arms control and disarmament is good or bad, and only nuclear experts are fit to decide whether, when, and where nuclear power plants should be built. There are today many scientists and engineers, and many members of the general public as well, who believe that basic issues like these are simply beyond the ken of the people and their elected representatives, and that public policy concerning such matters should indeed be made by a scientific technological elite. Eisenhower's second warning is even more pertinent today than it was when he made it.

As fate would have it I worked fairly closely with Eisenhower during the last three years of his presidency, first as a member of the Science Advisory Committee he set up immediately after Sput

nik under the chairmanship of James R. Killian Jr, and second as the first Director of Defense Research and Engineering, a new position created in 1958 as another part of the response to Sputnik. In these positions, I was directly concerned with precisely those scientific and technological programs in which the President himself was most involved and my own view of the world gradually changed as I came to see and understand the overall situation in which we found ourselves. I had gone to Washington a technological optimist, full of confidence in the technological fix. I came away three and a half years later gravely concerned about the all too common practice of seeking and using technological palliatives to cover over serious persistent underlying political and social problems. In particular, I became convinced of the futility of always devoting our main efforts to finding a technical solution to the problem posed by the steady decrease in our national security that was being brought about by the spread of high technology weapons throughout the world. This, it seemed to me, was not only futile but basically absurd, because nearly all of the weapons which in the hands of others were (and are) threatening our national security, and indeed our very existence, had been invented or perfected by us in the first place. In sum, my views on the relationship between technology and security did not arise out of Eisenhower's warnings; rather his warnings and my views both arose out of the same set of circumstances, but his formal warnings did very much help to crystallize my views on the subject. I found it very reassuring that the Commander-in-Chief, a professional military man himself, shared my own growing doubts about the value and efficacy of placing such a relatively high priority on finding technical solutions to what were really political problems.

Eisenhower's warnings, which were based largely on his remarkable intuition, pointed up very real and extremely serious problems. If we forget or downgrade his warnings, it will be to our peril.

HERBERT F. YORK
University of California, San Diego
La Jolla, California

• • •

Abridged version of the author's response on receipt of the Forum on Physics and Society Award on 27 April 1976. Further discussion of this subject may be found in York's recently published book "Race to Oblivion" reviewed in December (page 19).

Ether drift tested

This letter is in response to the letters of H. C. Dudley (February 1975, page 73) and Dale C. Scheetz (March 1976, page 15). Both letters address themselves to the question of the detectability versus the non-detectability of "ether-drift." The first letter suggests the possibility of using lasers or masers to provide useful data regarding "ether-drift." I should like to point to the paper by T. S. Jaseja, A. Javan, J. Murray and C. H. Townes[1] as a possible candidate. The fact that neither of the above-mentioned correspondents mentioned the work of Jaseja *et al* might be viewed as an oversight. This experiment used "one-way" light paths of two cross-fired infrared masers and drew the conclusion that there was no effect greater than 1/1000 of the v^2/c^2 term, over a period of six consecutive hours.

Once again you are vindicated, Albert!

Reference

1. T. S. Jaseja, A. Javan, J. Murray and C. H. Townes, Test of Special Relativity or of the Isotropy of Space by Use of Infrared Masers, Physical Review, 133, A1221 (1964).

J. W. HASLETT
University of Illinois at Chicago Circle
3/25/76 *Chicago*

Thermodynamic paradoxes

The article by Frank Weinhold on "Thermodynamics and Geometry" (March, page 23) gives an interesting new representation of an old branch of physics. However, I wish to point out that the formulation in terms of Riemannian geometry with a positive-definite metric is somewhat more restrictive than the second law of thermodynamics. The second law states that the entropy of an isolated system is maximized at equilibrium.[1] Since the energy of an isolated system cannot vary, the second law by itself says nothing about how the energy U varies with entropy, or with any other extrinsic variable X_i. Therefore, the second law does not require

$$|\mathcal{R}_i|^2 \equiv \left(\frac{\partial R_i}{\partial X_i}\right)_{X_1 \ldots X_{i-1} X_{i+1} \ldots X_{c+2}} \equiv \left(\frac{\partial^2 U}{\partial X_i^{\,2}}\right)_{X_1 \ldots X_{i-1} X_{i+1} \ldots X_{c+2}} \geq 0 \quad (1)$$

as stated in the article (page 26).

In general, the stability criterion (equation 1) is necessary only if one assumes a strong version of the zeroth law of thermodynamics, namely that two isolated systems each in stable equilibrium at the same temperature (or another intensive variable R_i) will be in stable equilibrium if placed in thermal contact (or contact for exchange of another extensive variable X_i). However, if equation 1 is violated, the two systems may be unstable to the transfer of entropy (or another X_i) when in contact, without there being an instability for either system when in isolation with the extensive

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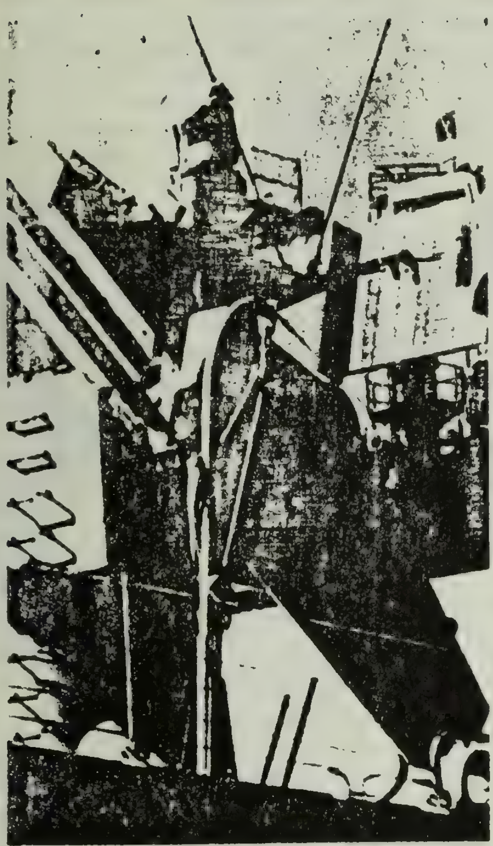


The mounting prospects of nuclear war

A report of the Stockholm International Peace Research Institute

Frank Barnaby

The probability of a nuclear world war is steadily increasing. If just the consequences of recent advances in military technology and the world-wide spread of this technology are considered, this conclusion is virtually inescapable. But there are other reasons for this pessimistic conclusion. Some of the main ones are:

- the arms race is now leading to a first-strike capability* by both the United States and the Soviet Union,
- the growth of peaceful nuclear technology is spreading the capability of producing nuclear weapons all over the globe,
- the international trade in arms is rapidly militarizing the entire globe,
- and, finally, the current arms control approaches have failed. They have failed to restrain the nuclear arms race; they have failed to prevent the proliferation of nuclear explosives, and they have failed to control the arms trade—let alone lead to nuclear disarmament.

Given the catastrophic nature of a nuclear world war, this increasing probability of its occurrence is, to say the least, alarming.

* * *

Quantitatively, both the United States and the Soviet Union have enormous strategic nuclear arsenals. The United States admits to having 2,124 strategic nuclear delivery systems: 1,054 land-based intercontinental ballistic missiles (ICBMs); 656 submarine-launched ballistic missiles (SLBMs), on 41 strategic nuclear submarines; and 414 strategic bombers. The U.S. arsenal can deliver about 8,500 independently targetable nuclear warheads.*

The Soviet Union is thought to have 2,404 strategic nuclear delivery systems: 1,452 ICBMs; 812 SLBMs on 60 strategic nuclear submarines, and about 140 strategic bombers. The Soviet arsenal can deliver about 4,000 independently targetable nuclear warheads.*

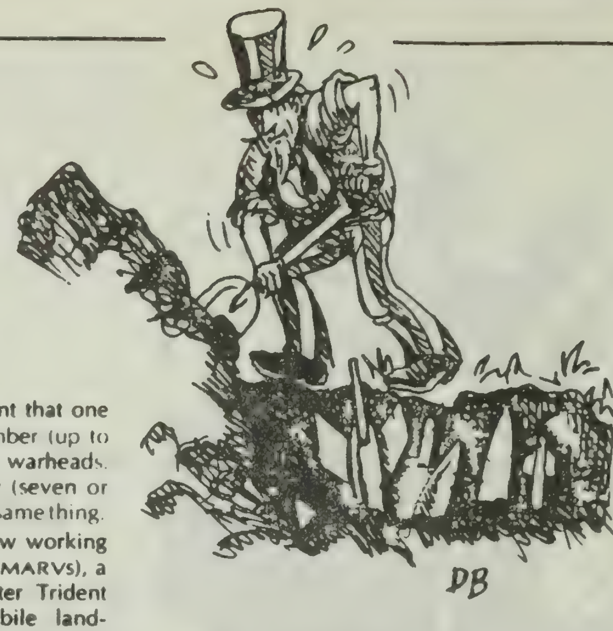
And, in addition to their 12,000 or more strategic nuclear warheads, the United States and the Soviet Union have tens of thousands of tactical nuclear weapons in their arsenals, mostly much more powerful than the atomic bomb that destroyed Hiroshima.

But, as awesome as these numbers are, recent *qualitative* developments in offensive and defensive strategic weapons and delivery systems are as dangerous, if not more so, than the size of these nuclear arsenals.

*A first-strike capability does not mean the ability of one side to destroy totally the other side's ability to retaliate. It means that one side perceives that it has the capability of destroying enough of the other side's retaliatory forces so as to limit the casualties and damage it would suffer from a retaliatory strike to an "acceptable" level for a given political goal. The more reckless the political and military leaders, the higher this level is likely to be.

*The numbers quoted are for July 1, 1976.

Time to bury 'deterrence'



William Epstein

Most observers are agreed that the balance of terror—that is, the mutual nuclear deterrence between the United States and the Soviet Union—has saved the world from a major East-West war and a possible global nuclear holocaust during the past quarter of a century. Nuclear deterrence is based on the concept of mutual assured destruction (with the lovely acronym MAD), which means that no matter which side strikes first and no matter how heavy the nuclear attack, the other side will retain enough strategic nuclear weapons to utterly destroy the attacker.

In the 1940s and early 1950s, before the Soviet Union developed a sophisticated nuclear arsenal, the United States was capable of carrying out a disarming or pre-emptive first-strike on the Soviet Union. But after the development of intercontinental ballistic missiles (ICBMs), in the late 1950s, that was no longer possible and attention turned to developing a second-strike or invulnerable retaliatory capability.

Thereafter the United States built concrete underground silos for its land-based ICBMs and nuclear submarines carrying submarine launched ballistic missiles (SLBMs). The Soviet Union, of course, followed suit but after a time lag of some five to seven years.

Then the Soviet Union started building anti-ballistic missile (ABM) defenses. The United States not only copied and improved on the idea of ABMs, it also developed multiple independently targetable re-entry vehicles (MIRVs), which meant that one missile could carry a number (up to 14) of separately targeted warheads. The Soviet Union is now (seven or eight years later) doing the same thing.

The United States is now working on maneuverable MIRVs (MARVs), a new generation of monster Trident submarines, a new mobile land-based missile and a new generation of B-1 bombers. It is also developing small long-range cruise missiles which can be launched from planes, submarines, and land or surface sea vessels, and which will fly low so as to avoid radar detection and be able to "read" the terrain and zero in on its target.

The Soviet Union, which developed big missiles with heavy warheads and greater throw-weight and is now busy MIRVing them, will unquestionably also copy the new American weapons or develop its own substitutes as they have always done in the past. And so the merry race goes on.

It goes on even though each side has developed massive overkill capacity: The United States now has sufficient strategic weapons to knock out every Soviet city with a population over 100,000 about 50 times; the Soviet Union is somewhat behind, as it can only knock out every equivalent American city some 20 times.

The SALT talks were supposed to limit and reduce the strategic nuclear arms race. The two superpowers did agree to limit ABMs to 100 on each side, although the United States has given them up entirely as they are not much good anyway. But the limitation of ABMs was supposed to "stabilize the deterrent," since neither side could acquire a defense against a nuclear attack, and thus MAD was saved. Each side is supposedly also deterred from launching a nuclear attack because it cannot prevent the other side from destroying it.

But is that necessarily true?

The SALT agreements put no limitation or restriction on the technological or qualitative nuclear arms race. They only provided numerical or quantitative ceilings for offensive nuclear weapons. Each side is now engaged in a technological race for the maximum improvement or perfection of its deterrent. And so the nuclear arms race is proceeding apace and moving in the direction of infinity killing power whereas the purpose of strategic arms limitation negotiations should be to move in the direction of zero killing power.

As President Carter said in his inaugural address, the SALT negotiations should be a first step toward the elimination of all nuclear weapons.

If the nuclear arms race continues with its present MAD momentum and with the development of new counterforce doctrines and a whole spectrum of new, more accurate and more powerful weapons, one side or the other, or both, may come to suspect that the other side is aiming at nuclear superiority so that it could win a nuclear war. Any fears that the other side was seeking to achieve a disarming first-strike capability—impossible as that may be because of the existence of the invulnerable nuclear submarines (each one of which could devastate either the United States or the Soviet Union)—could create a highly destabilizing and dangerous situation.

Even on a bi-polar basis mutual deterrence tends to lock the parties into a position of structured opposi-

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tion. The policy of deterrence becomes an end in itself with self-perpetuating vested interests and a dynamic and momentum of its own.

It is no doubt true that with the development of détente, the thawing of the Cold War and the current situation of stable deterrence, the likelihood of a deliberate planned war between the United States and the Soviet Union is receding. But as the nuclear arsenals grow, with a greater variety of weapons, the danger of a nuclear war by inadvertence grows. A nuclear war could be unleashed as a result of human or mechanical failure, by accident, by miscalculation (because of ineffective command, control and communications procedures or capabilities), by the escalation of a local conventional war, by blackmail or terrorism, or by sheer madness.

So long as the leaders of both the United States and the Soviet Union always behave in a rational and humane way, deterrence may continue to work. But if the leaders on either side make any political, technical, administrative or military nuclear mistake for any reason whatsoever, whether real or imagined, the entire system could break down, with a real possibility of the destruction of the civilized world.

Bad as the present situation is, it would become infinitely worse, if nuclear weapons proliferate and we have to live in a world of many nuclear powers. Small or middle-sized nuclear powers would not have the same sophisticated second-strike capability as the great nuclear powers. In an acute crisis one of them might fear that a nuclear adversary might launch a pre-emptive first-strike against it, and in order to prevent that possibility might itself decide to strike first.

Moreover, the smaller nuclear powers would probably not have sophisticated command, control and communications facilities or the electronic safety locks and other advanced physical security arrangements. Thus the possibilities of accident, misinterpretation of orders, theft of nuclear weapons, nuclear terrorism and blackmail would be greatly multiplied in a proliferated nuclear world.

Whatever validity the doctrine of mutual deterrence might have in a bi-polar world or in a world of a few nuclear powers, it would become meaningless in a world of many nuclear powers with great differences in their geopolitical, military and technological positions as well as in the number, sophistication and vulnerability of their nuclear arsenals. If one could work out all the permutations and combinations of the possible ways that an inadvertent nuclear war could begin, the probability of its happening sooner or later would become almost a certainty. Deterrence never worked for long and always broke down in the era of conventional arms. There are no grounds for thinking that it will work indefinitely in the nuclear age.

The Alternative?

What then is the alternative to seeking security on the basis of mutual deterrence?

Difficult though the answer may prove to be, there is only one sure road to greater security—namely, to eliminate all nuclear weapons. If the justification for each side's nuclear arsenal is the existence of the other side's nuclear arsenal, then there is no justification for the existence of both arsenals. Both should be eliminated. Jimmy Carter is right—we have to move toward the elimination of all nuclear weapons.

Most members of the arms control community are convinced that, unless some way is somehow found—and soon—to halt and reverse the nuclear arms race, a nuclear war is almost inevitable. The main points of difference are not "whether" but "when" and "how" it will happen.

But President Carter has provided us with a glimmer of hope that sanity will prevail and that the superpowers will stop playing nuclear roulette with human survival. He wants to stop all nuclear testing, to "freeze" the production of new nuclear weapons and new generations of existing ones, to cut down the tremendous stockpiles of nuclear weapons and to move in the direction of their complete elimination. He also wants a tougher policy on nuclear exports by all countries in order to prevent the proliferation of nuclear weapons. His new SALT negotiator and Director of the Arms Control and Disarmament Agency, Paul Warnke, is also willing to undertake unilateral initiatives for restraint in developing and producing nuclear weapons, on a temporary trial basis, to see if that will stimulate a reciprocal Soviet response that could lead to a mutual agreement.

If Carter's new policies are to succeed and not get sidetracked or undermined by the military-industrial-scientific-bureaucratic complex, he will need strong support from the U.S. allies as well as from domestic and world public opinion.

Another hopeful development was the decision of the U.N. General Assembly last December to hold a special session on disarmament in May-June 1978 and for a preparatory committee to start work on it this spring. If the major nations, and above all the United States, are willing to make the preparatory committee a serious and important means to re-think and re-plan a new approach to a saner world, then the special session can become a turning point in the efforts to halt the race to oblivion.

To make the special session a success, it is necessary that the preparatory committee be regarded as providing a new opportunity and that it not be limited to purely procedural preparatory work or to the old worn-out concepts. It should undertake a fundamental re-examination in depth of all the accepted shibboleths including deterrence, verification, military superiority and the relationship between the military bombs and the poverty, population, and pollution bombs.

It should not concentrate on studies of limited incremental arms control and limitation measures which, despite 7 multilateral treaties and 10 bilateral American-Soviet agreements in the last 13 years, have failed to slow down the arms race in any significant way, let alone halt it.

It should undertake broader studies of comprehensive disarmament programs including both nuclear and conventional weapons, of reciprocated measures of unilateral restraint, of across-the-board budget reductions, of the impact on the arms race and disarmament of the explosive advances in technology, and of the broader questions of the link between disarmament and a new world order.

If advantage is taken of new opportunities provided by the new look in Washington and by the U.N. special session, then there is a good chance for human survival in the nuclear age. But if this new hope is allowed to erode and decay into another lost opportunity then we are probably all lost.

Union of Concerned Scientists
1208 Massachusetts Avenue
Cambridge, Massachusetts 02138

September, 1977

Dear Colleague:

It is now thirty years since nuclear explosions devastated Hiroshima and Nagasaki. The thirty years have seen a continuing nuclear arms race between the superpowers, the United States and the Soviet Union. This arms race proceeds unabated in spite of the accumulation of weapons inventories of immense destructive potential. The continuation of this race poses a major threat to mankind.

We are writing to invite you to join us in supporting a proposal that the United States take new steps to control this race. We propose to issue on behalf of many members of the technical community and of other knowledgeable people a statement that may help the nation's leaders make decisions that can constitute important steps toward the needed control. The steps could be the beginning of nuclear arms reduction.

If you share our concern over the threat posed by the ongoing nuclear arms race and wish to join us in sending the enclosed Declaration to the President and to the Congress, please return the enclosed form.

Sincerely,

Hans A. Bethe	Mary I. Bunting	Owen Chamberlain
Bernard T. Feld	J. William Fulbright	John Kenneth Galbraith
Donald A. Glaser	Donald F. Hornig	Carl Kaysen
Henry W. Kendall	James R. Killian, Jr.	George B. Kistiakowsky
Salvador E. Luria	J. Carson Mark	Eugene McCarthy
Linus Pauling	George W. Rathjens	Herbert Scoville, Jr.
Albert Szent-Gyorgyi	Victor F. Weisskopf	Jerome B. Wiesner

DECLARATION
ON
THE NUCLEAR ARMS RACE

I. The Arms Race

The nuclear arms race, a grim feature of modern life, has been with us since the devastation of Hiroshima and Nagasaki. Driven by a futile search for security through nuclear "superiority," this vastly expensive competition is increasingly a mortal threat to all humanity. Control of the arms race is one of mankind's most urgent needs.

The inventories of nuclear warheads are coupled with accurate, long range and relatively invulnerable delivery systems. Together their destructive capacity is immense. If used in war they would kill hundreds of millions of persons, carry radioactive injury and death to many of the world's nations, profoundly damage the environment of the Earth we live and depend on, and unhinge and devastate the target nations so effectively that they would no longer function as modern industrial states. Indeed, the superpowers' inventories are so great that even small fractions of them could produce damage far beyond any past experience.

Neither superpower could now launch a counterforce surprise nuclear attack to disarm the other. Enough of each side's forces will survive or are invulnerable that the return blow would still produce prodigious damage, so shattering that no first strike is remotely rewarding. Thus, a relatively stable but uneasy balance has resulted in the recent past, the state of Mutually Assured Destruction. This balance of terror, while morally repugnant to many, is a fact of modern life. The superpowers have recognized that the populations of the United States and the Soviet Union have become unavoidably hostage because of the ineffectiveness of defenses against nuclear-armed strategic weapons systems-- and so their 1972 agreement, and treaty, in effect terminated efforts at active anti-missile defenses.

The security of the United States and the Soviet Union, and of the other nations across the globe, is only as high as the expectation that the nuclear arsenals will never be used.

Strategic nuclear arsenals could be drastically reduced while still retaining ample deterrent forces in a world devoid of real peace. However, the superpowers--while professing commitment to the principle of nuclear parity--continue to reach for nuclear superiority or at least for unilateral advantage through the development and deployment of new weapons and weapons systems. By and large the U.S. has been ahead in the superpowers' arms technology race: the first in nuclear and thermonuclear weapons, nuclear submarines, solid fuel missiles, and many other developments. The U.S. continues to forge ahead: developing MX--an advanced, perhaps mobile, land-based intercontinental missile and multiple independently-targetable reentry vehicles (MIRV) of extreme accuracy, the neutron bomb, the air-and sea-launched strategic range cruise missiles.

Many of these innovations have been stimulated by uncertainty about what the Soviet Union was deploying. Soviet responses are clouded in secrecy and are possibly highly threatening. They have forced the U.S. to proceed. In general, the Soviet Union has responded to U.S. technological innovations but with a lag--averaging over 4 years--in reaching U.S. levels. Their deployments then continue, exceeding the United States' and so raising fears of their gaining "superiority." The Soviet Union is developing and deploying MIRV'ed missiles of ever greater range, accuracy, and explosive power, perhaps greatly intensifying the civil defense of its population, and continuing other developments. The Soviet Union now has more strategic missiles and a greater nuclear throw-weight, while the United States exceeds in the accuracy of delivery systems as well as in numbers of nuclear warheads. The Soviets continue also to increase their conventional forces raising fears of aggression aimed at Western Europe. This has stimulated responses in conventional arms, and, especially grave, in dependence on nuclear weapons among the NATO nations. The United States and the Soviet Union both are engaged in vigorous underground nuclear warhead test programs. The responsibility for the race is unmistakably shared.

The arms race is in full swing! The roughly twelve thousand strategic warheads of today are likely to become thirty thousand long before the end of the century and the tens of thousands of tactical weapons augmented also. These increases and the improvements in missile accuracy, retargeting capability and invulnerability lead to greater "flexibility"--and so to the greater likelihood of starting nuclear weapons' use. What results is the undermining of the balance of terror. New weapons now in sight will further decrease the stability of this delicate balance and will make the monitoring of future arms agreements more difficult, if not impossible, without gaining decisive military superiority for either side.

The superpowers' belief that security rests with potent nuclear armaments is more and more shared by other nations. The strategic arms race stimulates the proliferation of nuclear weapons among nations some of which may be weak or irresponsible, and thus more likely to resort to the use of nuclear weapons in a local war. Such wars could easily widen, thus adding to the likelihood of a final immense nuclear holocaust between the superpowers.

More than ever it is urgent now to slow down and ultimately to stop the nuclear arms race, thus improving the stability of the nuclear stand off and setting the stage for reduction of the great inventories of weapons.

II. Controlling the Arms Race

Several attempts have been made to bring the nuclear arms race under control but none has been successful in the face of the pressures that drive the competition. The 1960 treaty to demilitarize the Antarctic continent, the partial nuclear test ban of 1963 and the later treaties not to deploy nuclear warheads on the ocean bottoms and in outer space are but peripheral to the nuclear arms race. The Non-Proliferation Treaty of 1968 has not gained universal adherence and the superpowers have not carried out their implicit commitments in the treaty to seek nuclear disarmament. The U.S. and Soviet Union have negotiated bilaterally in the Strategic Arms Limitations Talks, or SALT, with some yield: they have agreed not to interfere with their non-intrusive means of verification of missile launch inventories, to minimize the installation of anti-ballistic missile systems, and they have placed ceilings, albeit very high ceilings, on the numbers of deployed strategic missiles. The talks have provided a forum for continuing negotiations, but they have *not* stopped the qualitative arms race, and they have *not* reduced the huge inventories of strategic delivery weapon systems. While negotiations advance slowly, hindered by mistrust and endless maneuver for advantage, virtually unlimited competition for "strategic advantage"--through new and more deadly delivery sytems--continues unhindered.

We believe that the key to a safer future lies in the *control of strategic weapons technology.* To protect the world from the disaster of a nuclear war the superpowers *must halt* the development of new weapons which frustrate attempts to curb the arms race. Because there is essential equality in U.S. and Soviet Union forces the superpowers *can still take effective steps* to stop the nuclear arms race. This must be done both through mutually agreed on, *and* through unilateral initiative actions.

III. Recommendations

We hereby recommend that:

1. The United States announce that it will halt underground testing of nuclear explosives provided that the Soviet Union follows suit within a reasonable time.
2. The United States announce that it will not field test or deploy *new* strategic nuclear weapons, nuclear weapons systems, or missile defense systems for a period of 2 to 3 years provided the Soviet Union demonstrably does likewise.

These measures, carried out with due care, *do not jeopardize our security.* The recommendations do not stem from blind faith in the good intentions of the Soviet Union. We already can detect Soviet tests of nuclear weapons smaller than the Hiroshima bomb with existing seismic sensors and clearly distinguish them from earthquakes. Hence underground tests of strategic warheads cannot escape detection. Our satellites already inspect Soviet missile launches, missile site construction, and submarine arrivals and departures; thus we would know if the Soviet Union were not following our lead. Should the recommended initiatives not bear fruit, the interruption in testing would hardly degrade our security. It takes many years to develop and deploy strategic weapons systems and our strength is such that a short delay of the sort we recommend cannot put the U.S. at risk.

These measures, carried out with due care, *can restrain the technological arms race.* Without underground tests there is not enough confidence in the new warhead designs to allow deployment. New missiles also depend on more accurate guidance systems, and these can only be tried and perfected in repeated test firings. By reducing the number of missile test firings to those needed for maintenance a major hurdle to new deployments would be created.

This is the moment for such moves. We are, once again, at a turning point in the nuclear arms race. Because SALT I succeeded in placing ceilings on the number of missile launchers, it stimulated an intense race towards more accurate and powerful missiles, and more warheads per launcher, the development of new and more potent bombers and submarines to replace existing fleets. Most importantly President Carter has displayed a more penetrating understanding of the dangers of the arms race than the previous leaders of the U.S. and U.S.S.R., and has indicated a readiness to consider imaginative policies. Our recommendations do not only meet a current need--they come at a propitious moment.

The United States should take the initiative. The U.S. lead in new weapons technology in the nuclear era is a reflection of our overall superiority in creating new technologies and sophisticated industries. Under these circumstances, we cannot expect the U.S.S.R. to take the initiative.

Our proposals would be *an important step* toward the controls of strategic weapons technology which are so essential to our short term security. They would thereby create that base of confidence and stability which is a prerequisite to overall reductions of the nuclear arsenals.

We urge the government of the United States to demonstrate its dedication to arms control by initiating the unilateral, reciprocal steps we have recommended, that represent the first steps leading to gradual disarmament. These actions, if carried out by the United States, would represent a policy of restraint of the greatest political significance and yet, for an interim period, be devoid of military risk. Should the Soviet Union reciprocate--and they, like the United States, have much to gain in so doing--a great step forward would be taken to diminish the threat of nuclear war.

This Declaration was prepared under the auspices of the Union of Concerned Scientists.

A peril and a hope

With the discovery of fission mankind entrained cosmic forces with human irrationality; we must face the problem of reducing the tens of thousands of nuclear bombs ready to be released in seconds.

Victor F. Weisskopf

Forty years ago Otto Hahn and Fritz Strassmann discovered fission. The existence of this phenomenon could have been predicted before it was found if the theoretical physicists at that time had shown a little more inventiveness: It is a simple consequence of the facts that the Coulomb repulsion increases with the square of the number of protons and the nuclear attraction only with the first power. In 1938 enough was known about the nuclear force that it would not have been too hard to conclude that the nucleus must become unstable against a split in two parts around atomic number $Z = 90$.

Although the fission process itself was not foreseen, the possibility of a nuclear chain reaction was indeed thought of in the early 1930's by Leo Szilard. The implementation of such a reaction with fission would depend upon the number of neutrons released in the process. This number was not easily predictable; even today it would be hard to determine it theoretically from our knowledge of the nuclear forces. That this number is considerably larger than unity does not appear to be based upon any fundamental property of the nuclear forces. There are no very deep reasons; it might have been less than one, but it was not.

Cosmic fire

The fact that the number of neutrons emitted per fission is around two seems to be of very minor importance to Mother Nature. Apparently the only major roles of the fission process are to provide an upper limit to Z and to influence to some extent the abundance of fission products. Apart from this, we may forget about fission if we are interested only in the major features of our world. This is even more true about the chain reaction itself. Nature has not made much use of it; recently evidence was found that a natural chain reaction happened a billion years ago below the soil of Africa; but, to our knowledge, nuclear chain reactions have never played any role in the development of our Universe.

Do we humans count in this Universe? If we do, my previous statement is totally wrong. For humankind the existence of this chain reaction is of decisive importance, probably greater than we can fathom today. We know that fission can be used destructively as an explosive and constructively as a source of useful energy. It is therefore a very effective instrument of power, both political power and physical power.

This, in itself, is nothing new in the history of the natural sciences. New discoveries have led to new weapons, to new energy sources, and to countless applications from which a lot of good has emerged—as well as some results that have not been so good. (In the last 20 years it has been fashionable to emphasize the "not so good," but let us be objective and fair.)

There is indeed something different in the latest developments of physics, which I will call "the leap into the cosmos." Previously we were dealing mostly with processes similar to those occurring in our terrestrial environment. In the last few decades, however, we have taken a decisive step: We now deal with extraterrestrial phenomena. Nuclear physics and subnuclear physics deal with the excitation of quantum states that are beyond the reach of ordinary terrestrial energy exchanges: In general, nuclear reactions do not take place on Earth. Nuclear dynamics is dormant in our environment. It is of course true that natural radioactivity occurs on Earth, producing the heat in the depths of our planet, but these radioactive elements are the remnants of a very different age and of a different environment: They are the last embers of the cosmic explosion that produced terrestrial matter.

Today we physicists deal with cosmic processes in which many millions of electron volts per atom are exchanged rather than the few electron volts that are customary here on Earth. Of these cosmic processes, the fission chain reaction was one of the first to lead to major technological applications. Two hundred million electron volts per atom, twenty million times more than the most powerful chemical reaction. This is cosmic and not ordinary fire.

The first major application was a destructive one, which ended World War II by killing a quarter of a million people with two bombs. It is not surprising, therefore, that people are fearful and bewildered, and that they have misgivings even in regard to the more benign applications. The arm of technology grew by a factor of a million within the lifetime of one generation.

In 1940, however, we took little time to speculate on these questions. The discovery of fission came at a dark time in the history of mankind. Germany was in the grip of a collective mental disease of unparalleled virulence. The whole world was threatened by the expanding cancer of Nazism; the Germans discovered fission, and they might have used it if it had been usable at that time. Many of us physicists—those who were not yet too deeply involved in the development of radar, which saved England from being destroyed—worked hard to improve our

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understanding of fission. Many of us hoped that the number of neutrons per fission would be low enough to prevent the making of a bomb. But it was not.

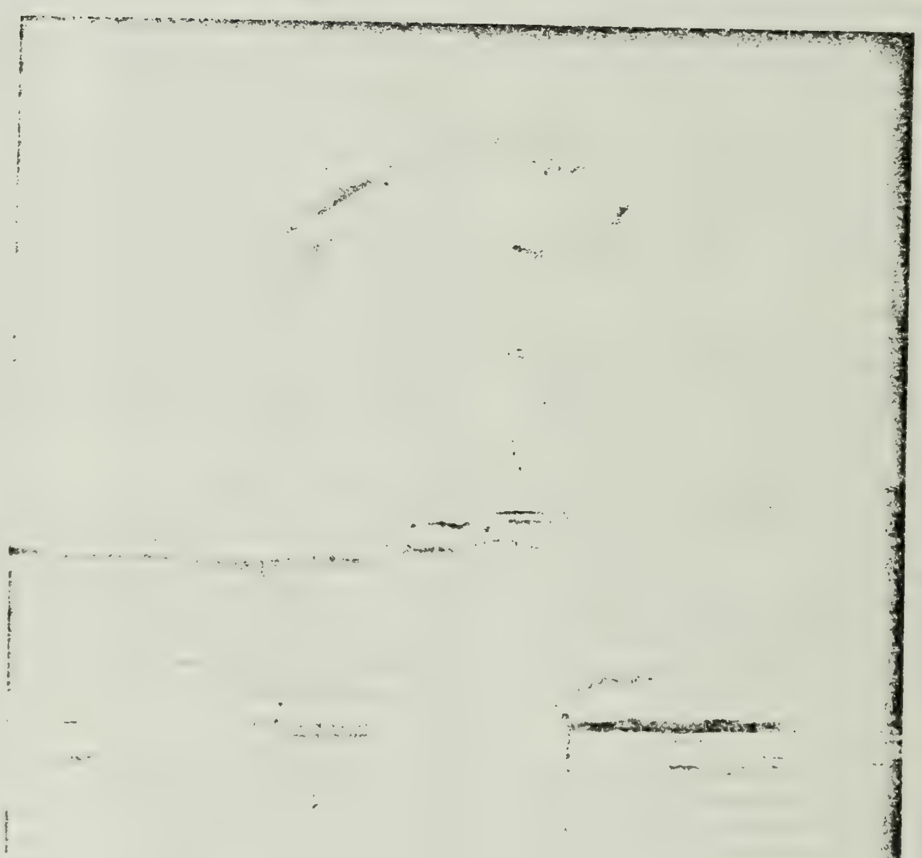
History takes a turn

A tremendous collective effort began. Within two years, the first nuclear chain reaction was produced under the leadership of Enrico Fermi, and within another three years, with the collaboration of many European physicists—in a truly international effort—a nuclear bomb was developed. On 16 July 1945 the first nuclear explosion was set off in a desert of southern New Mexico, at a place called Jornada del Muerte (a Spanish army perished there two hundred years ago). Human ingenuity had succeeded in the release of cosmic forces that were hidden and unknown 13 years earlier, when James Chadwick discovered the neutron.

Some of us saw this event, which, at our observation post, had the intensity of twenty midday suns: an expanding fire ball, white, and then yellow and orange, rising majestically into the sky, surrounded by a halo of blue light. The air was fluorescent with radioactive radiation.

Here it was: The laws of Nature did admit an explosive nuclear chain reaction. Because of some little detail in the equations of nuclear matter, the number of neutrons per fission was large enough. The history of mankind took a turn.

From then on, political developments, and no longer scientific ones, determined the course of events. Two bombs were exploded over Japanese cities; over 200 000 people lost their lives. Why two bombs? Why over populated areas? I am not sure I can answer these fateful questions. The bombs ended the war; they made an armed invasion unnecessary



US AIR FORCE

"Fat Man," the plutonium bomb dropped on Nagasaki, Japan on 9 August 1945. Larger than "Little Boy," the uranium bomb detonated over Hiroshima, it had about the same yield, some 20 000 tons of TNT. The production of nuclear chain reactions gave Man control over cosmic forces.

and thus perhaps saved more people than they killed. But Japan may have given up anyway under the pressure of a near defeat and a Soviet declaration of war. Whatever the answer, the stigma has remained upon the United States, as the first and only country to have used nuclear bombs. I can not tell you whether the decisions were right or wrong—I don't know.

The war ended, and it was physics that had helped to win it, with radar and the atom bomb, as well as many other new gadgets. Physics and the physicists moved onto center stage in public life; the significance of basic physical science became generally recognized. New means of research, technical and financial, suddenly were available to physicists. The radar technology created sophisticated new methods for producing electromagnetic waves of all kinds. Maser and laser physics emerged from it, and with it a deeper understanding of the fundamental interactions of light and matter, leading to quantum electrodynamics, as well as of the structure of solids under various extreme conditions.

Nuclear and subnuclear research experienced an almost explosive development, with an ever growing array of cyclotrons, synchrotrons and linear accelerators. The cosmic scope of physical research widened and penetrated into new realms of phenomena:

- On the microscopic side, into the GeV region—with its mesons, hyperons, antiparticles, heavy electrons and quarks, entering the innermost structure of matter;
- On the macroscopic side, into plasma physics, space physics, a new cosmology and astrophysics—with its quasars and pulsars, and the observation of the optical reverberation of the birth of the Universe, the famous three-degree radiation in space.

The physicists experienced a tremendous surge of support for their activities. This in turn led to an unparalleled expansion of their fields of endeavor and to a large range of new technical applications, both destructive and constructive, such as fusion bombs, rocketry, transistors, space travel, computers and many more. The physicists now moved to the center of public attention. It was their enthusiasm and self-confidence, spurred by their successes during and after the war, that brought them into contact with the great social and political problems of the times.

Many tried, and some succeeded, to bridge the "communication gap" between politicians and scientists. Attracted by great, optimistic ideas of how to establish a new order in the world they had helped to engender, they embraced the idea of international control of nuclear explosives. They hoped this would stop all future wars, and direct the applications of fission and fusion away from destructive bombs and towards an unlimited benign source of energy for all mankind. Some of them, perhaps, also experienced a reaction against their feeling of guilt for having been involved in the creation of a device that could annihilate all mankind.

Power—and anxiety

The political realities, however, were not conducive to the ideals of those who tried to unite the nations. Stalinism in the Soviet Union, and nationalism all over the world, including our own country, led to a breakdown of attempts to lift nuclear matters beyond national sovereignty. Of course, neither the physicists nor the politicians had much experience in dealing with such complicated technical matters on an international scale. With the development of fusion bombs, the nuclear arms race began in earnest.

Among the benign implications of nuclear physics, there still remained the great promise of unlimited energy through the use of fission reactors and, perhaps later, through controlled fusion. But over the decades following World War II, doubts were raised even on that account. Because the yield per atom is so many million times higher than in any conventional way of producing power, the consequences of accidents caused by human error are much worse. A nuclear power station certainly can not explode like a bomb, but the possibility of accidents that could spread large amounts of radioactivity can not be completely excluded. Even if human ingenuity and care can keep the accident rate at an acceptable level, the public and some of the experts look with some anxiety at this "extraterrestrial" way of producing energy in our terrestrial abode. The fact that the public is acquainted with this cosmic force only through the experience of nuclear bombs strengthens this anxiety. Furthermore, the use of fission for power constantly creates new raw material for bombs, which, in a nationally and politically divided world, adds to the danger of further spread of nuclear weapons.

The dangers and the promises of nuclear-power generators are today in the center of discussion; many studies have been undertaken and more are under way. Emotion and vested interest unfortunately have led to a sharp division of opinions, and the arguments used on both sides are too often beyond the limit of dignified scientific discourse.

Apotheosis of irrationality

At the same time the nuclear arms race between the superpowers continues in an almost uncontrolled way. The Soviet Union and the United States assemble increasing numbers of bombs, and perfect their efficiency and their mode of delivery. More than 50 000 nuclear bombs are deployed and ready for use. Each country now has the capability of destroying the other many times over. Current science is totally unprepared to discuss intelligently, let alone to predict, the totality of horrors that would result from an all-out nuclear war. Consider only the effects of the vast amounts of radioactivity released upon our environment; these would be so devastating that the condition of life would be permanently and dangerously altered, without much hope of recovery. Even the detonation of a single weapon of modern design over a city would be a catastrophe unprecedented in human his-

tory. Yet two large countries keep building more and more of these horrendous means of annihilation, knowing well that any actual use of these devilish gadgets would mean certain destruction of a large part of the world—making it unfit for habitation, with little chance of a recovery of civilization. Why? Why? Why? Only because neither side knows where to stop, and both go on producing nuclear weapons intended for all sorts of imagined missions. Only because each party is under the grip of an unrealistic measure-countermeasure syndrome. It is the apotheosis of irrationality and antilogic; it is the triumph of craziness.

In comparison to this overwhelming threat the nuclear power controversy dwindles to picayune dimensions. What are the dangers of nuclear-power stations compared with the dangers of tens of thousands of bombs that can be released within seconds by a small group of human beings? What is the so-called "worst reactor accident" compared to nuclear war? The damages done by the former, which would come mainly from effects of radiation—serious as they are—are far less than the effects of a single bomb. Now think of the number of victims of a nuclear war and its irreparable effects on our environment, on our souls, alive or dead, and on our planet as a whole.

And the probabilities? Nobody really can estimate the probability of an all-out nuclear war, but one fact is clear: With all those bombs around, it is not zero. Nuclear power may be too risky, or it may not; I do not pretend to know the answer. But I *know* that tens of thousands of stored bombs are too risky.

Undoubtedly, it is extremely improbable that the US or the Soviet government will decide outright to set off a nuclear war, thereby annihilating both countries simultaneously. However, a nuclear clash may develop from local wars between smaller countries that have acquired, or will soon acquire, a few nuclear weapons, and may make use of them in desperation. Or it may develop from an escalation of the use of so-called "tactical" nuclear weapons, which are meant to be applied in defense against aggression by conventional weapons. The tens of thousands of nuclear bombs are a mighty tinderbox; they may explode whether it was directly intended or not.

HIROSHIMA PEACE MEMORIAL MUSEUM

Nothing obscures the view south from the roof of a building around Yamaguchi cho. The city of Hiroshima has become a bare scorched field; Motoujina and a gas tank appear to be near at hand.

Is there a way out?

I am only too well aware of the difficulties in the way of reducing that danger. I know also that the presence of these weapons is given credit for having prevented a world war for a longer period than ever before. But, as time passes, the weapons multiply and become more efficient; they are adapted to all kinds of purposes, such as the neutron bomb. They are therefore more likely to be used. The outbreak of a volcano becomes more violent after long pauses. Here we have a man-made volcano: It could be removed.

I used the phrase "almost uncontrolled" in referring to the nuclear arms race. There have indeed been a few hopeful attempts, albeit small and tentative, that slowed it down a bit. One was the cessation of bomb tests above ground; the other was the first SALT agreement six years ago, which will, I hope, be expanded in the near future. They are not much, but they are two small steps in the right direction, and we can be proud that some of our colleagues were active in bringing them about.

The difficulties of going further are enormous. Some raise the fear that the other side will gain more, or might even dare to take the tremendous risk of a first strike regardless of the consequences. There are those who say that a free society can not compete in terms of nuclear armaments with an authoritarian system in any other way but through an all-out technological arms race. Voices are heard that we should not be too confident of negotiated contracts with our opponents; that we should rather assure a reasonable survival rate by a large effort of organized civil defense in order to make the population as ready as one can be for the great holocaust. I honor the intentions of those who advocate such measures but, to make them effective, if this is at all possible, would thoroughly change our way of life. I can not help sympathizing with the majority of our population, who do not want to live under a constant awareness of mankind's ultimate self-destruction.

We can not go on forever living under a continuous threat of annihilation. There must be ways and means to decrease the number of nuclear bombs. Certainly this task can not be accomplished from today to tomorrow; it is bound to be an extremely difficult process.

The foremost problem

There is a hope and not only a peril in the nuclear development. The growing recognition of this awful threat may still change the attitudes of the sovereign states, to lead slowly to the recognition that ultimate military preparedness is much less safe than a reduction and eventual abolition of nuclear weapons. J. Robert Oppenheimer said in 1945, shortly after the end of the war:

> "The point is that atomic weapons constitute a new field and new opportunities ... when people talk of the fact that this is not only a great peril, but a great hope, this is what they should mean. I do not think they should mean the unknown, though sure, value of industrial and scientific virtues of atomic energy, but rather the simple fact that, because it is a threat, because it is a peril and because it has special characteristics, there exists a possibility of realizing, of beginning to realize, those changes which are needed if there is to be any peace."

The task must be faced; it is the first and foremost problem of our time. Any one of us can and should play a role in this task, as a scientist and as a human being. The most important step is a new setting of priorities. The reduction and eventual abolition of nuclear weapons *must have absolute priority*; everything else must be subordinated to this goal. The consequences of nuclear war are irreparable, whereas the consequences of other setbacks in world politics can be corrected.

HAJIME MIYATAKE HIROSHIMA PEACE MEMORIAL MUSEUM

A victim of the atom bomb exploded over Hiroshima. Over 200 000 people lost their lives in the two Japanese cities bombed in World War II. Today more than 50 000 nuclear bombs vastly more powerful than these are deployed and ready for use, and the inventories are growing every day. **Weisskopf:** "The reduction and abolition of nuclear weapons must have absolute priority."

This change of priority is essential and can be achieved only by constant pressure of public opinion. Remember how effective public opinion was during the Vietnam War. In nuclear matters, the public is now interested only in the relatively unimportant issue of nuclear power. This must change. The issue of getting rid of nuclear armaments must receive much more public attention, support and pressure than it receives today. There is much too little discussion of these essential questions: Do we not already have enough bombs for deterrence? How would the Soviet Union react to a restraint in development or to a cutback? We must find that right balance of risks most conducive to lowering the levels of nuclear armaments. What is needed is a combination of new technical ideas and common sense, based upon humane considerations.

Only when we see a chance of success in the abolition of nuclear armaments can we scientists be proud of the achievements we gained during the last decades: our leaps into the cosmos; our penetration into the innermost structure of matter; our achievements in getting at the basic processes of life, the workings of DNA and RNA, and the tremendous developments in our knowledge of organized matter.

It is not the first time in history that human greatness and human folly grew side by side in the same period. Think of the Gothic cathedrals, together with the senseless and murderous crusades, 700 years ago. Think of the blossoming of art and philosophy during the Renaissance, along with the decimation of Europe's population during the religious wars, 500 years ago. Think of the music of Mozart and Beethoven, and of the slave ships plying the oceans, 150 years ago. Think of the greatest achievements of scientific thought, quantum mechanics and relativity theory, and the ascendancy of the murderous periods of Nazism, Fascism and other authoritarian regimes, 50 years ago. Finally, think of the great achievements of science today, together with the folly of the nuclear arms race.

The last folly is more serious than all the previous ones. We are dealing with cosmic forces. Our epoch may be the end of what has been a great age of mankind, great in spite of all the strife and wanton destruction. Our age has been great in its achievements in art, architecture, literature, music; great in its numerous social innovations, in spite of the fact that in most parts of the world social organization and the quality of human life leaves much to be desired; great in its medical successes, which have resulted in the doubling of the average age of man; great in its means of food production, communication and transportation—which makes a united world without hunger and want a possibility, if not a reality. And, last but not least, ours has been a great civilization because of the constant growth of our insights into the mysteries of Nature, the continuous opening up, leaf by leaf, of the blossoms of truth and wonder.

The age of insight

If we do not succeed in abolishing the nuclear arms race and a nuclear war results, all these great steps will be brought to naught. The twentieth century would then be remembered as the time of preparation for the great catastrophe, and science would be seen as the main culprit and the main instrument of destruction. The twentieth century ought to be remembered as the age in which mankind acquired its widest and deepest insights into the Universe, and learned to control its martial impulses.

Let us hope, strive and act so that it will.

* * *

This article is an adaptation of a talk given 24 April 1978 at the Washington, D.C. meeting of The American Physical Society, as part of a commemorative session marking the fortieth anniversary of the discovery of nuclear fission. □

US STATE DEPARTMENT

To peace! At the SALT talks in Vladivostok, November 1974, Soviet premier Leonid Brezhnev and US President Gerald Ford raise their glasses in a toast. What is needed to find the "right balance of risks" are "new technical ideas and common sense based upon humane considerations."

Likelihood of Major Nuclear War:
estimated at once in 30 years.

Population at Risk:

139,826,023 (total)

NUCLEAR WEAPONS RISK

= 4,660,867 Casualties per year

Likelihood of Major N-Plant Accident:
estimated at once in 1,000,000 years.

Population at Risk (Deaths + Injuries):

48,000 (prompt) + 293,000 (delayed) = 341,000 (total)

NUCLEAR POWER RISK

= 0.3 Casualties per year

Comments on the Environmental Impact Statement for the Lawrence Livermore Laboratory

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The nuclear weapons problem is analogous to the nuclear power problem, in my opinion. Although clearly both the production of nuclear weapons and nuclear power could be made safer than they now are, I am convinced that nuclear weapons and nuclear power can never be made ethically acceptable in terms of public health and genetic integrity. Therefore, I favor getting rid of both the weapons and the power plants.

Efforts to make these twin technologies safer, make sense only if you consider both technologies acceptable and if you consent to having nuclear weapons and nuclear power on this Earth. If you do not consent, you avoid helping to perpetuate them with the "fix-it" myth---the sleeping pill for the public. Instead, you work to educate the public about your reasons---about those risks to health and life which can not be eliminated and which make the nuclear twins ethically unacceptable.

This hearing is a welcome step in that direction, for I am sure that most Californians have no idea what a hazard the Livermore Laboratory creates for them and for countless future generations. We should all be grateful to the Friends of the Earth and the Weapons Labs Conversion Project for helping to create this forum.

Now, specifically with respect to the Livermore Laboratory, I consider that the handling of kilo-curie quantities of plutonium and other actinides(indeed, the Environmental Impact Statement discusses a limit of 225 kilograms of plutonium for the Plutonium Materials Engineering Facility), means that the Livermore Laboratory and its DOE sponsors are quite willing to be responsible for causing from 10,000 to millions of lung-cancer deaths in residents of California, to render huge parts of California hostile for human habitation for hundreds or even thousands of years, and to pollute life even beyond California---for plutonium can migrate once it gets loose in the environment.

Both DOE and LLL have consistently downplayed the cancer-producing properties of plutonium, first by deceiving employes, and second by deceiving the public both through its employes and through publicity releases. In no way have DOE and LLL been able to refute my estimate that fallout plutonium itself has signed the death warrant of one million people in the Northern Hemisphere. The EIS models for calculating plutonium and other actinide dosage to the lungs are erroneous, and the DOE "Standards for Radiation Protection" are more appropriately described as "Licenses to Commit that Number of Random Murders Which Is Convenient for Its Operations". There is ample reason for Nuremberg Trials of DOE for its radiation standards on the basis of crimes against

humanity, but I shall not dwell upon that issue here. There are plentiful opportunities for that discussion in other forums.

As for the EIS descriptions of administrative controls, procedures of safety, redundancies, HEPA filters, etc. etc., they are not worth the paper they are written on. The Three-Mile-Island experience has properly convinced the majority of Americans that the nuclear bureaucracy is an Emperor without Any Clothes, and the Emperor's credibility will never be restored by pompous, assinine pronouncements of DOE, NRC, or nuclear officials.

So it must be made clear to all Californians and Americans in general that LLL and DOE regard them and their health and their property as expendable. That is the real meaning of the Environmental Impact Statement.

I know that some people suggest a solution for LLL via moving such work as its plutonium work to Nevada. The argument advanced is that Nevada is already thoroughly contaminated, and that the Nevadans have not objected to nuclear work. I consider that position immoral in the extreme. The Nevadans are not second-class citizens just because their population density is low. That they are ignorant of the hazards is <u>not</u> a reason to victimize them instead of victimizing Californians.

Now, having stated all this, I may surprise you when I state that I support the continued existence, temporarily at least, of the research and development effort on nuclear weapons conducted by DOE and LLL. I must explain, and I must measure my words carefully, for what I have to say may appear internally contradictory, although it is not so. There are other individuals and groups you will hear whose broad objectives are <u>not different</u> from mine, but who may easily misinterpret what I say.

A response to the EIS concerning the Lawrence Livermore Laboratory really must be predicated upon the values one holds. My hierarchy of values is the following:

<u>Highest:</u> Justice, Freedom from slavery, and Honesty.

<u>Next:</u> The right to life, and a healthful life, for this and future generations.

<u>Next:</u> The right to one's freedom of speech and the sanctity of one's property.

Because of the hierarchy, I hold that freedom of speech is forfeited if that freedom is used to advocate genocide, for instance, by Nazis. And I hold that the right to private property is forfeited if that property is being used to commit murder against the populace. Thus,

nuclear power plants---all of which are committing random, premeditated murder on members of the public---retain no sanctity against trespass. In both cases (speech and property), the sanctity of life is the higher value. But the nuclear weapons problem is one which involves a still higher value: freedom from enslavement.

It seems quite obvious, logically, that the whole concept of "deterring" nuclear war by having nuclear weapons, means that you believe at least one side has the evil intention to dominate or enslave the other. Otherwise, what is there to deter? Otherwise, why not just dis-arm unilaterally, regardless of what the other side does, if it's just a Pussycat? The moment you admit that we need some nuclear weapons, you admit there is some evil intent which needs deterring, and it follows from this admission that you have to work feverishly in your own weapons lab to stay-even-with, or ahead of the enemy.

Those who would suggest that the DOE-LLL efforts in nuclear research and development be abandoned, are in effect saying that it is safe to give the Soviet Union a first-strike capability on a silver platter.

There can be no doubt, without any recourse to classified information, that the essence of the U.S. and U.S.S.R. policy on nuclear weapons is research and development toward a first-strike capability. Sooner or later, one side will probably succeed in this endeavor, and will surely feel pressed to use that capability in a final solution.

As for the SALT agreements, I find those absurd, since the participants are willing only to give up numbers of weapons, not to stop the research and development to gain the crucial advantage. Thus SALT is an overt manifestation of mutual insincerity about preventing nuclear holocaust.

There is much insincerity in the U.S. posture. But we still have some freedom in the USA, an asset of immeasurable value. I have the most profound contempt for the state-slavery of the Soviet Union and the 1000-year Reich its leaders have imposed upon its unfortunate citizens. There are other areas of the world where the darkest of ages have also become reality. I want none of those slave-states able to enslave the people of the United States, and I have no doubt there are those, such as the Soviet Leadership, who would be willing to do just that. I do not consider this position as supportive of the Cold War, or of being a hawk. It is simply a matter of not being naive.

Thus, while DOE and LLL represent a deep threat to health and life through such activities as plutonium handling, I must reluctantly state that the higher value---freedom from slavery---necessarily is a more important value for me. Therefore, I do not advocate any unilateral interference with nuclear weapons R & D.

Nuclear weapons are indeed an abomination. Nuclear weapons R & D will, unchecked, lead to nuclear war. But the only hope, in my opinion, is to buy some time to open some new minds,with the hope that the world may come to its senses, and understand why we have this abominable threat.

The reason we have this threat is the existence of a medical disease known as power-lust. Worldwide, the sickest members of all societies, afflicted with this disease, scramble to the top of the heap and hold the world population hostage to their insane goals. The average American has no conflict with the average Soviet citizen, or citizen of any other country. The desperately sick people known as "leaders" or "rulers" play with supra-lethal toys, and threaten all of us throughout the world. Until and unless the people of the world learn the true threat from this sickness of their leaders, and remove them on a worldwide basis from the positions of power from which they can inflict so much misery, we are wasting our energies talking about SALT agreements, or moving Livermore plutonium work to Nevada, or nit-picking the technical details of the EIS document.

It debases our ethics and should boggle our minds to be here focusing on making preparation for nuclear war safer! What we need is some honesty---especially from Jimmy Carter, DOE, and the weaponeers---about where the nuclear R & D is inevitably leading, instead of the deception that SALT agreements will lessen the growing probability of nuclear holocaust.

Meanwhile, the best place to conduct plutonium work would be in a building attached to the United States Capitol, since nothing less will convince Congress of the moral bankruptcy of the U.S. with respect to weapons policies. I would equally favor seeing plutonium work in the Soviet Union performed inside the Kremlin, for the same reason.

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The Cancer Hazard from Inhaled Plutonium

John W. Gofman

May 14, 1975

CNR Report 1975-1 – R

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Foreword

The calculations presented here, and in the other reports of this CNR series, represent a first approximation of the biological hazards from plutonium exposure.

In essence, these are studies of the dosimetry of plutonium exposure. There are certain critical voids in mankind's knowledge of the physical and physiological parameters which determine the dosimetry, and thus we have made necessary assumptions which are all clearly identified.

It is anticipated that as additional data become available, the calculations herein will be updated to take them into account.

Summary of Conclusions

(1) The lung cancer potential in humans from inhaled insoluble compounds of plutonium (such as PuO_2 particles) has been grossly underestimated by such authoritative bodies as the International Commission on Radiological Protection and the British Medical Research Council.

(2) The term "lung cancer dose", used freely in this report, has a specific scientific definition, namely, the reciprocal of the lifetime lung cancer risk per unit of radiation, whatever be the units under discussion. In more popular terms, one "lung cancer dose" of a carcinogen such as plutonium introduced into a population will assure one extra lung cancer death.

(3) The lung cancer hazard of plutonium inhalation is much higher for cigarette-smoking humans than for non-smokers. The calculations presented here suggest the following values for inhalation of insoluble plutonium particulates.

For Cigarette-Smokers:

Pu^{239}

(a) 0.058 micrograms deposited Pu^{239} represents one "lung cancer dose".

(b) 7,830,000,000 "lung cancer doses" per pound of Pu^{239}.

Reactor-Pu

(a) 0.011 micrograms deposited reactor-Pu represents one "lung cancer dose"

(b) 42,300,000,000 "lung cancer doses" per pound of reactor-Pu.

For Non-Smokers:

Pu^{239}

(a) 7.3 micrograms deposited Pu^{239} represents one "lung cancer dose".

(b) 62,600,000 "lung cancer doses" per pound Pu^{239}.

Reactor-Pu

(a) 1.4 micrograms deposited reactor-Pu represents one "lung cancer dose".

(b) 338,000,000 "lung cancer doses" per pound of reactor-Pu.

(4) While the estimated hazard is about 127 times lower for non-smokers than for smokers, the hazard for non-smokers for reactor-Pu, which is what nuclear energy provides, is indeed severe. Clearly, there would be no source of comfort available even if no one in the population smoked cigarettes.

(5) The reason for the gross underestimate by ICRP or BMRC is their use of a totally unrealistic, "idealized" model for the clearance of deposited plutonium from the lungs and bronchi, plus their non-recognition of the bronchi as the true site for most human lung cancers. The erroneous model used by such organizations fails totally to take into account the effect of cigarette-smoking upon the physiological function of human lungs.

(6) Plutonium nuclides, or other alpha particle-emitting nuclides, in an insoluble form, represent an inhalation hazard in a class some five orders of magnitude more potent, weight for weight, than the potent chemical carcinogens.

(7) The beagle dog data on lung cancer production from inhaled plutonium already are in good general accord with the human estimates of this report, even though it is widely realized that the current beagle data must be overestimating the lung cancer dose. When the beagle data become available at lower dosages, it is virtually certain that they will not be significantly different from the human estimates.

(8) None of the calculations presented in this report make any use of "hot particle" theories and are in no way dependent upon such theories. Unfortunately, so much effort has been expended, for example, by the British Medical Research Council, in countering "hot particle" theories that they overlooked the real cancer hazard derivable from straightforward dosimetry, as presented here. It turns out that dosimetry provides cancer risk estimates well within order of magnitude agreement with those predicted by Geesaman-Tamplin-Cochran.

(9) The lung cancer potential of insoluble particles of plutonium compounds should result in worldwide rejection of nuclear fission energy involving any kind of plutonium handling or recycling. No meaningful mitigation of this problem would be achieved even if cigarette smoking stopped totally.

THE CANCER HAZARD FROM INHALED PLUTONIUM

John W. Gofman*

Introduction

At this critical juncture for societal choices of energy supply for the future, one possible choice is a nuclear fission economy based upon the element plutonium (element 94). Tamplin and Cochran [1] have pointed out that the U.S. AEC projected that over 4 million megawatts of nuclear capacity will be installed between 1970 and 2020. Based upon this estimate, Tamplin and Cochran pointed out that over the lifetime of these plants this installed capacity could result in a cumulative flow of approximately 200 million kilograms (440 million pounds) of plutonium through the nuclear fuel cycle. Putting this much plutonium "through the nuclear fuel cycle" means plutonium becomes a commonplace article of commerce, being handled by thousands of workers and being transported on highways, railways, and airways in numerous shipments per day.

Plutonium is widely recognized as a potent carcinogen, and is of particular concern in the form of insoluble particles of plutonium dioxide (PuO_2) as a very potent agent for the production of lung cancer in man. Estimates have been made by several individuals and groups of the number of human lung cancers to be expected for the inhalation of specified quantities of PuO_2 particles. Such estimates range over several orders of magnitude, with Cohen [2] providing the lowest estimate, Tamplin-Cochran [1] providing the highest estimate, and the British Medical Research Council [3] providing evidence suggesting an intermediate value. Unfortunately, the problem has been clouded by needless polemic discussion of whether or not the "hot particle" hypothesis (Geesaman) [4]

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is correct. The issue of PuO_2 particle carcinogenicity can be approached in a straightforward manner with no reference whatever to "hot particle" theories.

It is our purpose here to present such a straightforward analysis leading to some reasonable limits for the expected numbers of human lung cancers for the inhalation of plutonium particulates. There are certain crucial voids in our knowledge of the behavior and disposition of PuO_2, once deposited in the lung. As a result, the estimate of the number of cancers becomes dependent upon the assumptions used where evidence is lacking. Cohen, in his analysis, simply overlooked the important problems of behavior of the PuO_2 in the lung. The British Medical Research Council paid lip service to certain of the problems, but then neglected to indicate how failure to address the problems might provide a falsely low estimate of lung cancer hazard from plutonium inhalation.

Detailed Analysis of Lung Cancer Induction by Plutonium

The analysis of the lung cancer-producing properties of inhaled plutonium particulates (usually, but by no means necessarily, insoluble particles of PuO_2) proceeds by several steps.

Step 1: Analysis based upon the known carcinogenicity of x-rays, gamma rays, and neutrons for human lung tissue, followed by analysis of the dose to be delivered to lung tissue by inhaled particulates of plutonium, assuming the plutonium delivers its radiation to the entire mass of broncho-pulmonary tissue. Since the nuclear power industry will provide mixtures of plutonium nuclides, rather than the predominant nuclide, Pu^{239}, the analysis will consider effects of Pu^{239} and effects of Pu mixtures from nuclear power reactors, to be designated simply as "reactor-Pu".

Step 2: Analysis of the nature of the problem of non-uniform distribution of plutonium within the lung and the crucial problem of which cells in the broncho-pulmonary system are involved in human lung cancer production.

Step 3: Final estimates of the probable limits to be placed upon the lung cancer expectations per pound of plutonium deposited in lung tissue of human populations.

Step 1. Analysis based upon the known carcinogenicity of x-rays, gamma rays and neutrons for human lung tissue.

There has been, for several years, conclusive evidence that human population groups exposed to x-rays, or to combinations of x-rays, gamma rays, and neutrons have developed an excess of lung cancers that must be attributed to radiation exposure. Gofman and Tamplin(5-23) have presented comprehensive analysis of the quantitative aspects of such lung cancer production. More recently the BEIR Committee presented its analysis of the same evidence.(24) We shall discuss the differences (minor, at best) in the conclusions to be derived from both analyses. There is abundant additional proof that broncho-pulmonary irradiation produces lung cancer in man from the tragic experience of uranium (and other) miners exposed to radon gas and daughter products of radon. However, we shall refrain from using these latter data for quantitative purposes because virtually everyone realizes that dose estimation in rads or rems is exceedingly tenuous at best for the miners.

The analysis of Gofman-Tamplin led to the conclusion that, for diffuse lung irradiation, 1 rem means 2% increase over the spontaneous lung cancer death rate each year in an exposed population, once the latent period of some 10-15 years is passed.(25) Precisely how long this 2% per year increase (in lung cancer death rate) persists is not known from direct evidence. A modestly conservative estimate (agreed to by many observers) is a persistence of 30 years, but persistence for the remainder of the life span of the exposed population cannot be ruled out. The BEIR Committee recognized this uncertainty in its report. The value of 2% increase over spontaneous lung cancer death rate, according to Gofman-Tamplin, applies for young adults of the 20-30 year age range.

Some workers have analyzed lung (and other cancer) production by radiation in terms of the absolute number of cancers produced per rem exposure of a population, with no reference to a percent increase over the spontaneous occurrence rate. Cohen has chosen this approach, with the strange statement that:

> "It may be noted that our calculations employed the "absolute risk" model of Reference 5 (the BEIR report) rather than the "relative risk" model. Primarily this is because the age-dependent risk of each type of cancer is not readily available, and the calculations are more complex. In ref. 5, the relative risk model gives a two times larger effect. However, the available evidence tends to support the absolute risk model and it seems to be preferred by most experts in the field, so its use is justified by our aim to determine the most probable effects."

The available evidence, in the opinion of the present author, is very much in favor of the opposite conclusion - namely, that the relative risk method has very sound foundation indeed. A variety of pertinent sources of evidence points strongly to radiation action as a multiplier of other carcinogenic influences (e.g., radiation multiplies the effect of cigarette smoking in the uranium miners). If radiation acts as a multiplier, then the best approach is the relative risk method, with a specified percent increase over the spontaneous cancer fatality rate per rem of exposure. The BEIR Committee was unable to choose between the two approaches, commenting as follows: (p.99, BEIR Report)

> "Absolute risk estimates are generally more useful for purposes of radiation protection than are relative risk estimates, because they specify directly the number of persons affected. On the other hand, if the risk due to radiation were found to increase in proportion to the natural risk, then the relative risk would provide the more appropriate estimate. Since the existing knowledge of radiation carcinogenesis is not always sufficient to indicate which type of estimate applies best in a given situation, both the absolute risk (where possible) and the relative risk are given in this report."

Since the present author considers the scientific evidence overwhelmingly in favor of the relative risk method, that method (including BEIR's relative risk estimates)

will be used in all calculations here. It is a very simple matter, as will be noted, to convert the relative risk estimates, scientifically sound, into absolute numbers of lung cancer fatalities. Moreover, in periods of rapidly changing lung cancer death rates (such as the 1940-1975 period in the USA), the relative risk method can avoid serious errors of under-estimation. The absolute risk method, using data for populations exposed in or before 1945, may be truly irrelevant in making estimates for the real world of 1975.

The most recent datum from the American Cancer Society provides the estimated value, 63,500 lung cancer deaths per year (1975) for men in the USA.[26] Virtually all these lung cancer deaths are in men over 25 years of age, so they may be taken to occur in a population of approximately 50 million men (those over 25 years of age).

The spontaneous (or as BEIR calls it, the "natural") lung cancer death rate, therefore, is

$\frac{63500}{5 \times 10^7}$, or 1.27×10^{-3}/year. Expressed otherwise, this means 1.27 fatal lung cancers per 1000 persons per year, spontaneously occurring in men over 25 years of age.

If we now utilize the Gofman-Tamplin figure above of a 2% increase over the spontaneous rate per rem of exposure, we arrive, per rem, at the following:

$(0.02)\ (1.27 \times 10^{-3})$, or 2.54×10^{-5}/year as the expected increase in lung cancer fatalities per year per rem of exposure. Henceforth in this discussion, we shall refer to estimates arrived at in this manner as "Gofman-Tamplin" estimates.

The BEIR Committee arrived at a somewhat lower percentage increase per rem of exposure. However, BEIR realized that the exposed subjects had not been followed long enough to be sure they were on the "plateau" of observed effects. We may quote BEIR Report (p. 156) as follows:

> "It is possible, therefore, that in the final analysis the absolute risk in these groups will approach $2/10^6$/year/rem and the relative risk will reach 0.5% or higher. For the three groups (miners and Japanese

survivors) in which up-to-date information is available, it is significant that many new cases have been added during the past few years."

This is a powerful admission by the BEIR Committee. They are admitting that their estimate is only four-fold lower than Gofman-Tamplin and admitting that when all the evidence is in, they may be even closer to Gofman-Tamplin estimates.

Let us not anticipate the future, and simply proceed utilizing the BEIR figure of 0.5% increase in relative risk per year per rem, realizing that it is not a most conservative public health estimate. *

Since 0.5% is 1/4 of 2%, we would say that BEIR should conclude that the risk of fatal lung cancer, for USA subjects in 1975, is

$1/4 \times 2.54 \times 10^{-5}$/year/rem, or 6.3×10^{-6}/year/rem.

Henceforth in this discussion, we shall refer to estimates based upon this number as "BEIR" estimates.

Cohen, in his analysis, quotes BEIR as giving "The cancer risk of radiation to the lung as 1.3×10^{-6}/year-rem for adults". This low figure, based upon absolute data from 1945, may be truly irrelevant for exposure of populations today.

Henceforth in this discussion, we shall refer to Cohen's analysis based upon the 1.3×10^{-6} lung cancer deaths/year-rem as the "Cohen" estimate.

It was stated above that most observers (including BEIR Committee) consider the "plateau" effect may persist for 30 years, or even longer. And while not truly conservative (in the absence of positive knowledge), we shall, for present purposes, utilize the potential underestimate of 30 years on the "plateau".

This leads to the following total lung cancer production per rem as follows:

"Gofman-Tamplin"	$30 \times 2.54 \times 10^{-5} = 7.62 \times 10^{-4}$	lung cancer deaths per lifetime-man-rem.
"BEIR"	$30 \times 6.3 \times 10^{-6} = 1.89 \times 10^{-4}$	lung cancer deaths per lifetime-man-rem.
"Cohen"	$30 \times 1.3 \times 10^{-6} = 3.9 \times 10^{-5}$	lung cancer deaths per lifetime-man-rem.

*See Note 1 in "Supplemental Notes".

The Concept of the "Lung Cancer Dose"

It has become commonplace recently to alter such presentations of risk into another format, namely, that which describes "the lung cancer dose". This is a simple and useful way to present the estimates. If the lifetime risk is x per man-rem, then the "lung cancer dose" is (1/x) man-rems.

Thus, for illustration, if the lifetime risk is 1 out of 10, which means 0.1 per man-rem, then the "lung cancer dose" is 1/0.1, or 10 man-rem.

Applying this relationship to the estimates above, we derive the following:

	"Lung Cancer Dose", in man-rem
"Gofman-Tamplin"	$\frac{1}{7.62\times10^{-4}}$, or 1310 man-rem.
"BEIR"	$\frac{1}{1.89\times10^{-4}}$, or 5290 man-rem.
"Cohen"	$\frac{1}{3.9\times10^{-5}}$, or 25,600 man-rem.

Calculation (Step 1 level) of "Lung Cancer Dose" for Insoluble Inhaled Plutonium Particles

The Cohen approach (which we shall here term Step 1 level calculations) is to calculate plutonium dosage as though the dose were distributed throughout the entire mass of lung tissue. While it will be shown below (Step 2 calculation) why this is not reasonable, it will suffice for Step 1 calculations. Cohen has used the reasonable value of 570 grams as the lung mass for average man (exclusive of blood). Further, Cohen has applied a factor of 10 for conversion of rad to rem for the alpha-particle radiation of plutonium. The British Medical Research Council Report suggests (p. 10) a value of 10-20 for this conversion. Again, even though possibly not conservative enough, we shall use the Cohen value of 10 for conversion of rads to rems.

To convert microcuries of plutonium deposited to dose in rems, Cohen has used the equation:

Dose in rems/day = 51 EQ/M rems/day-microcurie

where E = energy deposited by alpha radiation in MEV

M = mass of organ in grams

Q = quality factor (the ratio of rems to rads, or the ratio of carcinogenic damage of the Pu alpha particles to that of gamma rays of the same energy).

(E for $Pu^{239} \cong$ 5.1 MEV; Q= 10; M $\cong$ 570 grams)

We shall accept all this for Step 1 calculation purposes, except to re-iterate that using M $\cong$ 570 grams assumes distribution of the dose to the whole lung tissue mass. In Step 2 calculations below, this crucial issue will be treated in detail.

Using the equation above, Cohen arrives at 2000 rems (for that portion of the plutonium presumed to be retained in the lung with a 500 day half-time for removal) per microcurie of <u>deposited</u> plutonium (Pu^{239}). We shall return later to this "500 day half-time for removal", but for Step 1 calculation, the 2000 rems per microcurie will be accepted. Incidentally, since the other Pu nuclides in "reactor-Pu" will have E values not very different from the 5.1 MEV for Pu^{239}, the same calculation will apply per microcurie of other Pu nuclides.

Since we have, above, assigned for the "Cohen" estimate, a value of 25,600 man-rem as the "lung cancer dose", it follows that 1 microcurie of Pu^{239} delivers $\frac{2000}{25600}$, or <u>0.08</u> "lung cancer doses".

Expressed otherwise, 1/0.08, or 12.5 microcuries Pu^{239} (For "Cohen" estimates) <u>deposited</u> provide <u>one</u> "lung cancer dose".

In Cohen's paper, he used a risk of "about 4.7 percent" per microcurie instead 8% per microcurie by including the risk for children (erroneously, we believe) and the

risk for adults well beyond 30 years of age. Since we are comparing all estimates for adults 20-30 years of age, we have made the minor adjustment in Cohen's estimate back to 8% per microcurie (as per his Figure 2).

Cohen stated, additionally, that of all the plutonium particulates inhaled, only 25% is retained for potential deposition, and he therefore multiplies his "lung cancer dose" by a factor of 4. Since all these discussions relate to deposited plutonium, rather than inhaled plutonium, it is inappropriate to utilize this particular factor of 4. Thus, we shall leave the "Cohen" estimate at 12.5 microcuries deposited Pu^{239} per "lung cancer dose" or per "lung cancer death".

There are 16.3 micrograms of Pu^{239} required to provide 1 microcurie of Pu^{239} alpha radiation. (This is directly calculable from the 24,000 year half-life of Pu^{239}).

Therefore, the "Cohen" estimate becomes

(12.5)(16.3), or 204 micrograms of Pu^{239} deposited per "lung cancer dose".

For "BEIR" estimate, with 5290 man-rems per "lung cancer dose", we calculate

$\frac{5290}{2000}$, or 2.65 microcuries Pu^{239} per "lung cancer dose".

Converting to micrograms, 2.65x16.3 = 43.2 micrograms Pu^{239} deposited per "lung cancer dose".

For "Gofman-Tamplin" estimate, with 1310 man-rems per "lung cancer dose", we calculate $\frac{1310}{2000}$, or 0.66 microcuries Pu^{239} deposited per "lung cancer dose".

Converting to micrograms, (0.66x16.3) ≅ 10.8 micrograms Pu^{239} deposited per "lung cancer dose".

All these data are summarized in Table 1.

Table 1

Step 1 Calculation

(assuming distribution of plutonium-239 throughout entire lung mass)

Estimate	Man-Rem per "Lung Cancer Dose"	Micrograms Pu^{239} deposited per "Lung Cancer Dose"
"Gofman-Tamplin"	1,310	10.8
"BEIR"	5,290	43.2
"Cohen"	25,600	204

Cohen has pointed out, correctly, that the mixture of plutonium nuclides from power reactors contains, in addition to Pu^{239}, several shorter-lived nuclides. Therefore, he states, correctly, that reactor-grade Pu is some 5.4 times as hazardous by weight as pure Pu^{239} (as high as 10 times in high burn-up light water-reactor fuel). Taking this 5.4-fold hazard factor into account, we arrive at the estimates in Table 2.

Table 2

Step 1 Calculation

(assuming "reactor-Pu" distributed throughout entire lung mass)

Estimate	Micrograms Reactor Grade Pu per "Lung Cancer Dose"
"Gofman-Tamplin"	2.0
"BEIR"	8.0
"Cohen"	37.8

We are now in a position to make a Step 1 estimate of the "lung cancer doses" per pound of Pu^{239} or per pound of reactor-grade Pu. The reader may well ask "Why calculate per pound of plutonium?" The answer is simply this. For considerations of the hazard posed by a plutonium-based nuclear fission energy economy, we know the number of pounds expected to be in daily commerce, and thus it is well to know the number of "lung cancer doses" involved in such an economy.*

*See Note 2 in "Supplemental Notes"

The calculation itself simply involves the number of micrograms per pound and the number of micrograms per "lung cancer dose".

1 pound $\simeq$ 454 grams

or, 1 pound = $4.54 \times 10^2 \times 10^6 = 4.54 \times 10^8$ micrograms.

Illustratively, we may calculate the number of "lung cancer doses" per pound of Pu^{239} in the form of insoluble PuO_2 particles or other finely dispersed insoluble Pu compounds.

From Table 1, for "Gofman-Tamplin" estimates, we have 10.8 micrograms Pu^{239} deposited per "lung cancer dose".

Therefore, "Lung Cancer Doses" per pound of $Pu^{239} = \frac{4.54 \times 10^8}{10.8}$, or 42,000,000 "lung cancer doses".

Similar calculations, for all three estimates, both for Pu^{239} and "reactor-Pu" are presented in Table 3.

Table 3

Step 1 Calculation

"Lung Cancer Doses" per pound of Plutonium

Estimate	"Lung Cancer Doses" per pound Pu^{239}	"Lung Cancer Doses" per pound Reactor-Pu
"Gofman-Tamplin"	42,000,000	227,000,000
"BEIR"	10,500,000	56,800,000
"Cohen"	2,225,000	12,000,000

It must be re-iterated here that all calculations of Step 1 type assume that the plutonium is distributed throughout the entire lung tissue mass. It does not assume uniformity of dose, but rather that the entire lung mass is the distribution region for the plutonium. As will be shown in Step 2 below, this may mean that the estimates of Table 3

represent minimum, rather than probable, estimates of the "lung cancer doses" per pound of plutonium.

Step 2: Analysis of the Nature of the Problem of Non-Uniform Distribution of Plutonium Within the Lung and the Crucial Problem of Which Cells in the Broncho-pulmonary System are Involved in Human Lung Cancer Production.

When a population is irradiated by x-rays, gamma-rays, or neutrons, and a dose is properly estimated for lung, we can expect, correctly, that the dose in rems to all segments of the lung-bronchus system is, to a good first approximation, everywhere identical. Under these circumstances it is reasonable to state that 570 grams of lung tissue have been irradiated. Even if some of the tissue (e.g. cartilage, smooth muscle, fibro-elastic support tissue) is not at all involved in cancer production, the dose estimate to the critical tissue susceptible to cancer production is still correct.

For inhaled particulate matter, estimation of the radiation dose as though the particles are distributed into 570 grams of lung tissue can be totally absurd. For example, it is extremely unlikely that any significant part of the inhaled particulates lodges in such tissues as bronchial cartilage, bronchial smooth muscle, walls of pulmonary arterio-venous network, or in fibro-elastic tissue. Therefore, the deposited particulates are distributed into some mass of tissue (including the critical cells for development of lung cancer) much less than 570 grams in mass. How much less? A reasonable first approximation, eliminating cartilage, fibro-elastic support tissue, arterial and venous walls, smooth muscles, and nerves, is that the relevant mass of tissue for distribution of the inhaled plutonium particulates cannot be more than 1/2 of 570 grams. Though this is just a beginning of Step 2 considerations, it immediately permits revision of Table 3 estimates upward by a factor of two. The revised results are presented in Table 4.

Table 4

(Preliminary Step 2 Calculation, based upon 0.5x570, or 285 grams as the Lung Tissue Mass)

"Lung Cancer Doses" per pound of Plutonium

Estimate	"Lung Cancer Doses" per pound Pu^{239}	"Lung Cancer Doses" per pound Reactor-Pu
"Gofman-Tamplin"	84,000,000	454,000,000
"BEIR"	21,000,000	113,600,000
"Cohen"	4,450,000	24,000,000

The Relevant Tissue for Lung Cancer Production

It is well known that the large preponderance of "lung" cancers arise in the bronchi rather than in the parenchymal lung tissue. Indeed it is this preponderance that accounts for lung cancer generally being referred to as bronchiogenic cancer. The BEIR report recognizes this, as does the British Medical Research Council Report. McCallum [27] states that such cancers are rare in the trachea or the two main-stem bronchi. The cancers are also relatively rare in the bronchioles. So the crucial tissue at risk must be the segmental bronchi, and, within these, the epithelial layer of the bronchi. What is really required is an estimate of the dose delivered by insoluble plutonium particles to this critical tissue, where almost all of the bronchiogenic cancers arise. The British Medical Research Council Report recognized this requirement, but in an apparent zeal for a pejorative analysis of the Geesaman-Tamplin-Cochran "hot particle" thesis, the B.M.R.C. report simply failed to address the most crucial problem of all.

The very fate of human societies may well rest upon this issue, considering the the proposed handling of some 440 million pounds of plutonium (and reactor-grade at that) in the next 50 years in a plutonium-based nuclear fission energy economy.

It is strange indeed that virtually all workers (Cohen, British Medical Research Council, ICRP, and others) have seemed fascinated by the 25% of inhaled plutonium deposited in the tissues beyond the bronchi, when virtually all the cancers arise in the bronchi. It almost seems as though the prevailing mood is that if a serious problem is simply neglected, it may disappear.

The Dose to Relevant Tissue

As will become evident below, it is no simple matter, in the current state of our ignorance, to calculate the true dose from insoluble PuO_2 particles to the relevant bronchial tissue.

All the above-mentioned groups or individuals have made use of a model for lung dynamics developed by a Task Group of the ICRP.[28] This model may be totally irrelevant for the question of exposure of the relevant bronchial cells. What does this model suggest, and where may it fail seriously in the real-life situation?

The model suggests that when PuO_2 particles are inhaled that some 8% deposits in the "tracheo-bronchial" region and some 25% deposits in the deep respiratory tissue ("pulmonary tissue"). It further assumes that the PuO_2 deposited in the tracheo-bronchial region is rapidly cleared into the intestine via the naso-pharynx, with 99% being cleared in less than a day. For the "pulmonary tissue" (tissue beyond the terminal bronchioles), the model suggests that 40% of the deposited PuO_2 is cleared in a day and 40% is cleared with a half-time of some 500 days. The remaining 20% is presumed, in the model, to be cleared via lymph and blood. The 80% (including the 40% rapidly cleared plus the 40% slowly cleared) are presumed to go back up through the tracheo-bronchial system to nasopharynx and thence to intestine.

This model is totally based upon the assumption of normally functioning epithelium of the bronchial system, particularly of normally functioning cilia* to propel the particles

*Cilia are specialized hair-like structures arising from the surface of lining cells, with the function of propelling material.

back up the tracheo-bronchial tree. If that ciliary function is impaired, then all the assumptions concerning clearance rate can be vastly in error, if applicable at all.

While the Task Group of ICRP was quite happy that the model seemed in reasonable accord with experimental animal data on a variety of particulate materials, the model may be irrelevant for humans in real-life circumstances.

Let us recall that most lung cancers (bronchiogenic cancers in man) occur in smokers of cigarettes. Roughly such cancers are 10x as likely in cigarette smokers than in non-smokers. This being the case, we really need to know what the circumstances of PuO_2 deposition and retention will be in cigarette smokers in the population, since this will overwhelmingly determine the bronchiogenic lung cancer effects.

In the extensive studies of lung cancer reported by the Surgeon General, [29] one outstanding set of facts was pointed out, based upon the work of Auerbach et al [30].

(a) There is considerable alteration of bronchial epithelium in cigarette smokers.

(b) There is a serious loss of ciliary presence in cigarette smokers (to say nothing of function of what cilia remain).

If our cigarette smokers have a serious loss of ciliary presence and function, of what use is a model that predicts clearances based upon intact ciliary function? We must face the possibility that, as a result of impairment or loss of ciliary function, PuO_2 deposited in the tracheo-bronchial epithelial region of man may be cleared extremely slowly. Further, the PuO_2 coming back up from the deep pulmonary tissue may also be hung up in the bronchial region, since it is assumed that the ciliary function is what propels it on, ultimately to the intestine.

It will indeed be no easy task to ascertain, for cigarette smoking humans, precisely what the clearance rates are for PuO_2 in human bronchial tissue. But it would

represent the height of public health irresponsibility either (a) to assume that an invalid, irrelevant model provides answers or (b) to neglect the problem simply because it is difficult.

An approach to Estimation of the Plutonium Dose to Relevant Bronchial Cells

There are two parts to this estimate:

(a) Estimation of the fraction of tracheo-bronchial region that is relevant for bronchiogenic cancer.

(b) Estimation of the clearance of PuO_2 particles by bronchial epithelium with impaired ciliary function, as in cigarette smoking humans.

(a) Estimation of the Relevant Part of the Tracheo-Bronchial Region

In the Task Group publication [28] it is estimated that the air volume of naso-pharynx plus tracheo-bronchial region down through terminal bronchioles is 133 cm^3, of which 50 cm^3 is assigned to the nasopharyngeal volume. This leaves 83 cm^3 for the entire tracheo-bronchial region. Since virtually no cancers arise in the trachea, we can subtract approximately 33 cm^3 for the tracheal volume, leaving 50 cm^3. The right and left main-stem bronchi (also rarely involved in cancer) represent a volume of approximately 11 cm^3, so this leaves 39 cm^3 for the bronchial region, including the terminal bronchioles. As a reasonable first approximation, 1/2 of this volume will be assigned to the relevant bronchi, and 1/2 to the volume of smaller bronchial branches plus bronchioles. Therefore, we have, finally, approximately 20 cm^3 for the volume in relevant bronchi. From Gray's Anatomy [31], the diameter of such intrapulmonary bronchi can be estimated as approximately 0.23 cm. (or radius = 0.115 cm.).

Treating these bronchi as cylindrical tubes,

Volume = $\pi r^2 h$, where h = equivalent length of total bronchi of this class

$$20 = 3.14\,(0.115)^2\,h$$

$$\text{or, } h = \frac{20}{(3.14)(0.013)} = \frac{20}{0.04} = 500 \text{ cm.}$$

To calculate the surface area of such bronchi, we use:

$$\text{Area} \approx 2\pi r h$$

$$\approx 2\pi(0.115)\,500$$

$$\approx (6.28)(0.115)\,(500)$$

$$\approx 361\ \text{cm}^2$$

A reasonable approximation for average height of the stratified columnar epithelium of these bronchi is 30 microns, or 3×10^{-3} cm.

Therefore, Volume of Epithelial Tissue $\approx$ Area x cell layer height

$$\approx 361 \times 3\times10^{-3} = 1083\times10^{-3}$$

$$\cong 1\ \text{cm}^3.$$

Since the density of soft tissue is $\sim$1 gram/cm^3, it follows that the mass of relevant bronchial tissue is $\sim$1 gram.

(b) Estimation of the Clearance of PuO_2 Particles by Bronchial Epithelium with Impaired Ciliary Function

The work of Auerbach et al (cited in the Surgeon General's Report on Smoking) shows the following severe losses of cilia in cigarette smokers (Table 5). These were controlled studies in which the pathologist did not know the smoking habits for the cases studied.

The Surgeon General's report comments as follows on loss of ciliary function (Ref. 29, pp 269-270:

> "Inhibition of ciliary motility following exposure to tobacco tars, cigarette smoke, or its constituents has been demonstrated frequently with experimental use of respiratory epithelium from a wide variety of animal species." (17 references quoted).
>
> "Similar results have been obtained with ciliated human respiratory epithelium." (2 references). "Although all investigations have been conducted *in vitro*, the uniformity of the inhibitory effects in a number of different experimental models is impressive."

Table 5 (Data of Auerbach et al)

Loss of Cilia and Epithelial Cell Abnormality

Group	# Cases	# Slides studied	Percent of slides with cilia absent and averaging 4 or more cell rows in depth.
Never Smoked regularly	65	3324	1.1%
Ex-Cigarette Smokers	72	3436	4.1%
Cigarettes - 1/2 pk/day	36	1824	4.7%
Cigarettes - 1/2-1 pk/day	59	3016	7.9%
Cigarettes - 1-2 pks/day	143	7062	16.9%
Cigarettes - 2+ pks/day	36	1787	37.5%

As noted in Table 5, over and above the loss of cilia there is marked abnormality in the epithelial layer of the bronchi.* Whether these altered epithelial cells may more avidly engulf PuO_2 particles than do normal epithelial cells, either by phagocytosis or endocytosis, is totally unknown. It is possible that the failure of clearance of PuO_2 by such regions may be seriously enhanced over and above the failure of clearance due to the absence of cilia. The Auerbach data reveal the absurdity of the model used by ICRP, by Cohen, and by BMRC for evaluation of PuO_2 clearance by the real population expected to be exposed to PuO_2 inhalation. In the heavy smokers, who will contribute most of the lung cancers, 37.5% of the cells have lost their cilia entirely. We can, therefore, with sound reason, presume that such regions of absent ciliary function will clear PuO_2 particles very slowly, if at all. It would not be at all conservative, for such regions, to assume that the half-time for clearance is 500 days for PuO_2 particles.

*Normal epithelium would show one or two cell rows in depth. Note that Table 5 describes the slides showing four or more cell rows in depth.

Quantitative Treatment of Smokers and Non-Smokers For Plutonium Lung Cancer Hazard

Inasmuch as the strong evidence indicates a different physiological handling of PuO_2 particulates for smokers versus non-smokers (henceforth, smokers will be considered to mean cigarette smokers), it is essential to consider these as separate sub-populations. The first step in such separate handling is to re-estimate the risks of lung cancer for smokers versus non-smokers.

For the overall male population (USA), the spontaneous lung cancer rate = 1.27×10^{-3}/year. (P.5, this report). Two subpopulations will be considered as a very reasonable approximation:

1/2 the men as non-smokers

1/2 the men as smokers (all cigarette smokers combined).

Let x = lung cancer rate for non-smokers

and 10x = lung cancer rate for smokers (P.15, this report).

Then, overall rate = $(1/2)\ (x) + 1/2\ (10x) = 1.27\times10^{-3}$

$$\text{or,}\qquad \frac{11x}{2} = 1.27\times10^{-3}$$

$$x = 0.23\times10^{-3}/\text{year}$$

$$10x = 2.3\times10^{-3}/\text{year}$$

With these evaluations of (x) and (10x), it is possible to convert all tables presented above into separate tables for smokers and for non-smokers. Wherever risks are involved, values for smokers (compared with overall population) must be multiplied by

$\dfrac{2.3\times10^{-3}}{1.27\times10^{-3}}$, or a factor of 1.81

Values for non-smokers (compared with overall population) must be multiplied by

$\dfrac{0.23\times10^{-3}}{1.27\times10^{-3}}$ or a factor of 0.181.

Table 3 can now be converted to one which treats smokers and non-smokers separately. These converted data, utilizing the factors above (1.81 and 0.181), are presented in Table 6.

Table 6

Step 1 Calculation: Separate Data for Smokers and Non-Smokers

Lung Cancer Doses per Pound of Plutonium*

For Cigarette Smokers:

Estimate	"Lung Cancer Doses" per pound Pu^{239}	"Lung Cancer Doses" per pound Reactor-Pu
"Gofman-Tamplin"	76,000,000	411,000,000
"BEIR"	19,000,000	103,000,000
"Cohen:	4,030,000	21,700,000

For Non-Smokers:

"Gofman-Tamplin"	7,600,000	41,100,000
"BEIR"	1,900,000	10,300,000
"Cohen"	403,000	2,170,000

*Note: This is still a Step 1 calculation assuming plutonium distributed into the entire 570 grams of (bloodless) lung tissue mass.

Step 3 Calculations of Lung Cancer Hazard from PuO_2 for Smokers and Non-Smokers

(a) The Cigarette Smokers:

As a result of the presence of large regions of cilia-free bronchi, coupled with potentially impaired ciliary function in additional regions, it is highly reasonable to estimate that clearance from cilia-free bronchial regions will be comparable with that estimated for cilia-free pulmonary regions. This leads to $T_{1/2}$ = 500 days for clearance for such cilia-free regions. From Table 5, cigarette-smokers of more than 1 pkg. per day average ~25% cilia-free regions.

Therefore, if we assume 25% of bronchi will show impaired clearance, we can hardly be overestimating the effect. It may not be conservative enough.

In the ICRP Task Group Model it is assumed that

25% of inhaled PuO_2 deposits in pulmonary tissue

8% of inhaled PuO_2 deposits in tracheobronchial region.

It is further assumed by ICRP that only 60% of the PuO_2 deposited in pulmonary tissue is retained for long-term clearance and that <u>none</u> of the PuO_2 deposited in the tracheobronchial tree is retained for long-term clearance.

With impaired ciliary clearance for 25% of the bronchial region, we shall assume

(a) that 25% of that deposited in tracheobronchial tree is subject to retention. 25% of 8% = 2% of total. Moreover, we shall use ICRP's estimate that 40% of this clears within a few days, leaving 0.6x2 = 1.2% for long-term retention.

(b) Further, of the 40% coming up rapidly (as per ICRP) from the pulmonary region that 25% of this 40% is retained in the bronchial region. 25% of (40% of 25%) = 2.5% is retained, additionally, of which 60% is retained <u>long-term.</u> Long term, therefore = 1.5%. Therefore, <u>total</u> retained for long-term clearance becomes

1.2 + 1.5 = 2.7% in bronchial region.

The ICRP Model allows 60% of 25%, or 15%, of total to be retained in <u>pulmonary</u> region, providing dose to this region.

Since we have just calculated 2.7% to be retained in the bronchial region, it follows that the bronchial region has a radiation source = $\frac{2.7}{15}$, or <u>0.18</u> as strong as the pulmonary region.

But to estimate <u>dose</u> to bronchial region, we must also incorporate the estimated tissue mass (bronchial) irradiated. This was shown above to be <u>one</u> gram.

Therefore, the overall radiation dose to bronchial region

$$= (0.18) \times \frac{(570)}{1} \times (\text{Dose to pulmonary region})$$

$$= (103) \times (\text{Dose to pulmonary region}).$$

This dose, in the cigarette smokers, will completely dominate the additional dose received by the pulmonary region.

It is now possible to estimate the lung cancer doses per pound of PuO_2 by applying this factor of 103 as a multiplier for all the values for smokers in Table 6. The results of this calculation are presented in Table 7.

Table 7

Step 3 Calculation: Final Estimate of PuO_2-induced Lung Cancers in Cigarette Smokers

Estimate	"Lung Cancer Doses" per pound Pu^{239}	"Lung Cancer Doses" per pound Reactor-Pu
"Gofman-Tamplin"	7,830,000,000	42,300,000,000
"BEIR"	1,960,000,000	10,600,000,000
"Cohen"	415,000,000	2,240,000,000

The number of micrograms Pu per lung cancer dose is now readily estimated for the cigarette smokers. For example, from Table 7, the Gofman-Tamplin estimate is 7,830,000,000 lung cancer doses per pound Pu^{239}.

$$1 \text{ pound} = 4.54 \times 10^8 \text{ micrograms}$$

$$\text{Therefore, } \frac{7.83\times10^9}{4.54\times10^8} = \text{lung cancer doses per microgram.}$$

The micrograms per lung cancer dose is the reciprocal, or $\frac{4.54\times10^8}{7.83\times10^9} = 0.058$ micrograms. In a similar fashion all the values of Table 7 can be treated to provide the estimates of Table 8.

Table 8

Final Step 3 Estimates of Micrograms Pu per Lung Cancer Dose in Cigarette Smokers

Estimate	Micrograms Pu^{239} per lung cancer dose	Micrograms Reactor-Pu per lung cancer dose
"Gofman-Tamplin"	0.058	0.011
"BEIR"	0.23	0.043
"Cohen"	1.10	0.203

(b) The Non-Smokers

For this population sub-group, the Auerbach data (Table 5) show the following:

Never Smoked Regularly: 1.1% of bronchial regions show cilia absent.

Ex-Cigarette Smokers: 4.1% of bronchial regions show cilia absent.

We shall weight the "never smoked" twice as heavily as the ex-cigarette smokers and arrive at a value of 2% as an average for bronchial regions showing cilia absent in a cross-section of non-smokers.

For the cigarette smokers, a value of 25% was used above for the bronchial regions showing ciliary absence. Therefore, we arrive at the estimate that, whatever dosage of the relevant bronchi is taken for cigarette smokers, the appropriate value for non-smokers is 2/25, or (0.08) of that dosage. The number of expected lung cancers from plutonium inhalation in non-smokers will therefore be(0.008)x(lung cancers expected in smokers). (0.08 for source strength and 0.1 for cigarette-lung cancer risk.)

Accordingly, Table 9, providing lung cancer doses per pound of Pu for non-smokers is derived from Table 7 by multiplying all values by (0.008).

Table 10, providing micrograms Pu per lung cancer dose, is derived from Table 8 by dividing all values by (0.008).

Table 9

Final Step 3 Estimates of PuO_2 Induced Lung Cancers per Pound in Non-Smokers

Estimate	Lung Cancer Doses per pound Pu^{239}	Lung Cancer Doses per pound Reactor-Pu
"Gofman-Tamplin"	62,600,000	338,000,000
"BEIR"	15,700,000	85,000,000
"Cohen"	3,300,000	17,900,000

Table 10

Final Step 3 Estimates of Micrograms Pu per Lung Cancer Dose in Non-Smokers

Estimate	Micrograms Pu^{239} per lung cancer dose	Micrograms Reactor-Pu per lung cancer dose
"Gofman-Tamplin"	7.3	1.4
"BEIR"	28.8	5.4
"Cohen"	138.0	25.4

In the treatment here, both for smokers and non-smokers, no crediting was given to diminished ciliary function over and above ciliary absence. It is quite possible that we have underestimated the hazard of plutonium inhalation as a result. Nevertheless, the preference is to understate the hazard wherever data are not absolutely firm, provided all concerned realize that there may well be an understatement.

GENERAL DISCUSSION

Are the Estimates Too High or Too Low?

It is evident from all the discussion up to this point that certain key parameters of physiological function are not available through direct experimental evidence for humans. One fact, however, is outstanding--that is the failure of authoritative bodies such as ICRP or BMRC to come to grips with the real-life problem of bronchopulmonary retention of PuO_2 particles in cigarette-smoking humans. This failure has led them to the use of a totally unrealistic and probably irrelevant model which drastically underestimates the lung cancer hazard of PuO_2 inhalation. One may ask whether the retention in bronchial tissue, secondary to loss of ciliary function, will really lead to a 500-day half-time for clearance of PuO_2 particles. We simply don't know, but it is just as reasonable to expect an even longer retention time as it is to hope for a shorter retention time. Since ciliary function is the mechanism counted upon for differentiating rapid clearance in the bronchi versus slow clearance in the pulmonary region, the absence of effective ciliary function makes it reasonable, as a first approximation, to expect clearance times to become identical.

If there be any *intrinsic* more rapid clearance mechanism (aside from cilia) for bronchial cells than for pulmonary cells, such mechanism is totally hypothetical. Indeed, the effect can be such as to worsen the estimates.

One may ask whether the metaplastic and hyperplastic epithelium of the bronchi of cigarette-smoking humans is more or less active in the engulfing of PuO_2 particles than is the normal epithelium. We simply don't know, but it is, *a priori*, equally probable that such epithelium can be less, equally or more active in engulfing PuO_2 particles. The burden of proof that metaplastic and/or hyperplastic epithelium is *less* active in engulfing PuO_2 particles would rest upon those who think it may be *less* active. From what we know about the general physiology of injured or inflammatory tissue, the expectation, if anything, is for greater phagocytic activity, not less. And this would make the PuO_2 carcinogenicity worse than calculated, not better.

The Hazard of Dispersal of Plutonium Oxide Aerosols

Cohen endeavored to show that plutonium dispersal was not as bad as general opinion has held it to be. The seriousness of his under-estimate of the cancer hazard of inhaled PuO_2 aerosols is evident in this report. Thus, comparison of Cohen"s 2,225,000 lung cancer doses per pound of Pu^{239} (Table 3) with the final "Gofman-Tamplin" estimate of 7,830,000,000 for cigarette-smoking humans, shows that Cohen is low by a factor of $\frac{7,830,000,000}{2,225,000}$, or 3520 times too low. Even for the non-smokers, his estimate is some 30 times too low.

In view of these serious under-estimates of the lung cancer hazard from inhaled Pu, most of his estimates of the hazard of plutonium dispersal will require scaling up by a factor of 3520 times.

Cohen, in his general thesis that plutonium, while very toxic, is not as toxic as many have thought, presented a calculation that insoluble reactor grade Pu is roughly

60 times more carcinogenic than benzpyrene. Benzpyrene is a now-famous substance, being one of the most potent chemical hydrocarbon carcinogens known. If we correct Cohen's estimate by the 3520 fold factor required, his estimate would then be that reactor grade Pu is roughly 211,000 times as carcinogenic as benzpyrene for smokers. It would seem that this revision would materially enhance the carcinogenic stature of plutonium for Cohen.

In consideration of "lung cancer doses" per pound of plutonium it must be recalled that this reflects the expected number of fatal lung cancers per pound of deposited plutonium. The question of how much of dispersed PuO_2 actually gets deposited is a wholly separate issue, based largely upon meteorology and dispersal conditions. Thus, if plutonium is dispersed and falls out over the ocean, there are few humans around to inhale it, so very few of the cancers can occur. On the other hand, dispersal with fallout in a city can lead to very drastic consequences in lung cancer fatalities.

It has been estimated that the nuclear weapons testing of the 1950s and 1960s has resulted in the worldwide fallout of some 11,500 pounds of plutonium-239 equivalent.[35] Some have suggested that if plutonium is so virulent a carcinogen as it appears to be, why haven't more cases of lung cancer occurred as a result of this fallout? The author has calculated the consequences of this plutonium fallout, and these consequences will be presented in a separate report.[32] Huntington has repeatedly raised the question of whether the increasing epidemic of lung cancer may be, in part, due to plutonium fallout.[33] Huntington may well have raised one of the most crucial public health issues of our time.

Comparison of Human Data with Experimental Beagle Data

The British Medical Research Council Report has reviewed the beagle dog studies of Bair and Thompson.[34] The initial depositions were between 3 nanocuries

and 50 nanocuries of Pu-239 per gram of bloodless lung. Even at the lowest level (3 nanocuries per gram of lung), essentially 100% of the dogs died of lung cancer. We know, therefore, that 3 nanocuries per gram of bloodless dog lung is <u>at least one</u> "lung cancer dose". As BMRC pointed out, the true "lung cancer dose" could be much lower, and on-going experiments at lower doses will be required to test this issue.

But, there are sufficient data already, to compare the beagle evidence with the human calculations presented in this report.

3 nanocuries per gram = 3×10^{-3} microcuries per gram.

To scale to human, with a 570 gram bloodless lung we have

$3 \times 10^{-3} \times 570$, or 1710×10^{-3}, or 1.7 microcuries of Pu-239 is <u>at least one</u> "lung cancer dose".

Conversion to micrograms, 1.7x16.3 = 27.1 micrograms of Pu-239 is <u>at least one</u> "lung cancer dose".

In Table 8, "Gofman-Tamplin" estimates are that 0.058 micrograms is the lung cancer dose for cigarette smoking humans, and in Table 10, the similar estimates are that 7.3. micrograms of Pu^{239} is the lung cancer dose for non-smoking humans.

Curiously enough it has been overlooked that beagle dogs raised in laboratories are not in the habit of smoking cigarettes. If a relevant comparison is to be made with humans, the appropriate treatment would be to compare the beagle data with the estimates for non-smoking humans.

Let us compare these values directly:

For the beagle dog (a non-smoker): 27.1 micrograms Pu^{239} is <u>at least</u> one lung cancer dose.

For the human (non-smoker): 7.3 micrograms Pu^{239} is one lung cancer dose.

As the BMRC report pointed out, virtually 100% of the beagles developed lung cancer at 3 nanocuries per gram of bloodless lung. It would be in the realm of miracles that the 3 nanocuries/gram happened to coincide with one lung cancer dose. In all likelihood, the true lung cancer dose for non-smoking beagle dogs is lower than 3 nanocuries per gram, and quite possibly considerably lower, just as was pointed out by the British Medical Research Council Report. Since the beagle data are even now so close to the estimates calculated here, it seems virtually certain that the newer beagle data will not be significantly different from the human estimates.

The Standards for "Permissible" Exposure to Plutonium, Occupational and for the Public-at-Large

The existing guidelines for "permissible" exposures to plutonium particulates permit:

(a) Occupational workers: Maximum lung burden = 0.016 microcuries.

(b) Public-at-Large: Permissible burden for the average person = 0.0005 microcuries.

Tamplin and Cochran[1], at the time of releasing their report, stated that the current guidelines make it extremely likely, indeed almost certain, that exposed individuals (occupationally-exposed) would develop fatal lung cancers.

It is of interest to test this prediction of Tamplin and Cochran against the calculations of this report, calculations that in no way depend upon the hot particle approach utilized by Tamplin and Cochran.

Predictions for Occupational Exposure

Since 16.3 micrograms represent 1 microcurie of Pu^{239}, the occupational permissible burden of 0.016 microcuries represents 0.26 micrograms of Pu^{239} equivalent.

For Cigarette-Smoking Workers:

1 lung cancer dose = 0.058 micrograms (Table 8). Therefore, each worker is permitted to acquire a lung burden of $\frac{0.26}{0.058}$, or 4.5 lung cancer doses. Since it only takes one lung cancer to kill a human, it is something of an overkill to guarantee 4.5 fatal lung cancers per worker. For these workers, therefore, we not only agree with Tamplin-Cochran, but we believe they understated the hazard.

For Non-Smoking Workers:

1 lung cancer dose = 7.3 micrograms (Table 10). Therefore, each worker is permitted to acquire a lung burden of $\frac{0.26}{7.3}$, or 0.036 lung cancer doses. Therefore, the expectation is that approximately one such worker out of thirty would develop fatal lung cancer at the permissible dose.

Predictions For the Public-at-Large

The implications of this report's calculations for the public-at-large are much more startling. The permissible average burden of 0.0005 microcuries of Pu^{239} corresponds to 0.0082 micrograms of Pu^{239} equivalent.

The population of the USA is roughly 1/2 non-smokers, 1/2 cigarette smokers. Since there are some 10^8 males per generation, at the current US population size, there are 5×10^7 cigarette smokers and 5×10^7 non-smokers.

Total lung cancer doses, for cigarette smokers,

$$5\times10^7 \times \frac{0.0082}{0.058} = 5\times10^7\times0.14 = 7\times10^6 \text{ lung cancer doses.}$$

Total lung cancer doses, for non-smokers,

$$5\times10^7 \times \frac{0.0082}{7.3} = 5\times10^7\times 0.0011 = 0.06\times10^6 \text{ lung cancer doses.}$$

Combining these, we have 7,060,000 extra fatal lung cancers that can be expected in USA males per generation if the population exposure to plutonium approached that which regulations now permit.

Since these lung cancers would occur over a 30-year period, the expectation would be for $\frac{7,060,000}{30}$, or 235,000 extra fatal cancers per year in men. Since the current lung cancer fatality rate, from all causes combined, is 63,500 per year in men, the conclusion must be drawn that governmental regulatory bodies are not disturbed over causing an additional four times as many lung cancer deaths as are now occurring.

Many serious public health experts consider 63,500 lung cancer fatalities per year to represent a most serious epidemic. How should they view the burgeoning plutonium-based nuclear fission energy economy, proceeding under regulatory standards that would permit a four-fold increase supplementary to this epidemic?

Supplemental Notes

Note 1: The BEIR relative risk percentage refers to adults. If it were restricted to 20-30 year old adults, the BEIR value might have to be increased even further than the 0.5% value used in this report (for BEIR). This entire present report, for consistency, compares all estimates for males in the 20-30 year age range (see p.3, this report).

Note 2: Calculation of fatal doses per pound of a toxic material of commerce may, at first glance, appear to represent an effort to exaggerate toxicity. This is incorrect. Indeed, it will be quite relevant, in the future, to describe all industrial pollutants in a similar manner. For substances handled in commerce in pound or ton quantities, a rational reference framework will be to require toxic or fatal doses per pound.

Some observers have pointed out that society has handled many highly toxic non-radioactive pollutants in pound or ton quantities. Since, in general, no careful followup studies have ever been made for most such pollutants, it may well be that a societal reappraisal of such non-radioactive pollutants is urgently indicated.

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Estimated Production of Human Lung Cancers by Plutonium from Worldwide Fallout

John W. Gofman

July 10, 1975

CNR Report 1975-2

Committee for Nuclear Responsibility, Inc.
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Foreword

The calculations presented here, and in the other reports of this CNR series, represent a first approximation of the biological hazards from plutonium exposure.

In essence, these are studies of the dosimetry of plutonium exposure. There are certain critical voids in mankind's knowledge of the physical and physiological parameters which determine the dosimetry, and thus we have made necessary assumptions which are all clearly identified.

It is anticipated that as additional data become available, the calculations herein will be updated to take them into account.

Summary of Conclusions

1. Worldwide fallout of plutonium-239 (and other plutonium nuclides) from past atmospheric weapons tests have produced a sizeable, and reasonably well estimated, deposition of plutonium in the lungs of inhabitants of the Northern Hemisphere.

2. Since the lung cancers expected per microgram of plutonium inhaled are available (Reference 1), it is a straightforward matter to estimate how many persons have been irreversibly committed to develop plutonium-induced fatal lung cancer.

3. For the USA alone, it is estimated that 116,000 persons have been committed to plutonium-induced lung cancer. In the entire Northern Hemisphere, the total number is $\sim$ 1,000,000 persons.

4. Since the latent period is over for a sizeable part of the plutonium fallout exposure, many of these estimated lung cancer fatalities must be occurring annually now. Probably in the entire Northern Hemisphere, of the order of 10,000 must be dying annually of plutonium-induced lung cancer.

5. Lung cancers, once induced, do not identify themselves as to cause. This is the reason that the absurd, although common, statement can be made that "cancers due to plutonium haven't been observed".

6. The experience of the small groups of Manhattan Project plutonium workers or the Rocky Flats plutonium workers is totally consistent with the expectations for plutonium-induced lung cancer presented here. By no means can these groups provide any comfort whatever for those hoping for a lesser carcinogenicity of inhaled plutonium.

7. Based upon the data presented here for fatal lung cancers already committed by weapons plutonium fallout in the USA, an estimate can be made for the future lung cancers to be produced by the developing nuclear power industry. If that industry contains its plutonium 99.99% perfectly, it will still be responsible for 500,000 additional fatal lung cancers annually. This would mean increasing the total death rate in the United States by 25% each year, since 2,000,000 persons currently die from all causes combined.

ESTIMATED PRODUCTION OF HUMAN LUNG CANCERS BY PLUTONIUM FROM WORLDWIDE WEAPONS-TEST FALLOUT

John W. Gofman*

Introduction:

Plutonium inhaled in the lung, particularly in the form of such insoluble particulates as plutonium dioxide (PuO_2), is one of the most potent lung cancer-producing agents known. Gofman has recently estimated the carcinogenicity of such particles both for smokers of cigarettes and for non-smokers[1]. The results are best expressed in "lung cancer doses", where one "lung cancer dose" is the reciprocal of the lifetime risk per unit of carcinogen. Thus, as an example, if the lifetime risk of lung cancer per deposited microgram of Pu^{239} is x, then the "lung cancer dose" is $(\frac{1}{x})$ micrograms.

For deposited Pu^{239}, the findings were:

For Cigarette Smokers (males), 0.058 μg. Pu^{239} = one lung cancer dose.
For Non-Smokers (males), 7.3 μg. Pu^{239} = one lung cancer dose.

Plutonium has several nuclides, so that it is important to specify whether pure Pu^{239} is at issue, or some mixture of nuclides. Cohen[14], for example, has estimated reasonably that usual reactor plutonium is 5.4 times as hazardous per microgram deposited in the lung, because of the admixture of shorter-lived plutonium nuclides. A convenient way to deal with unknown mixtures of plutonium nuclides is to determine the alpha particle activity in Curies (or some subunit such as picocuries) of Pu^{239} equivalent, and then convert to micrograms, utilizing

$$16.3 \text{ micrograms } Pu^{239} = 1 \text{ microcurie } Pu^{239}.$$

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As a result of worldwide fallout of plutonium from weapons tests conducted in the atmosphere, it is estimated that approximately 320,000 Curies of Pu^{239} equivalent received global dispersion and fallout.(2) Some part of this fallout was inhaled by humans, particularly in the Northern Hemisphere, and is now part of the measured body burden of plutonium observed. In view of the extremely high lung cancer potential of plutonium inhalation, it is important to evaluate how many lung cancer fatalities are currently being caused by inhaled fallout plutonium and how many cases are to be expected in the future.

As will become evident in the body of this report, the plutonium inhaled from worldwide weapons test fallout may have already created, irreversibly, one of the prime public health problems of our era.

Analysis of the Lung Cancer Induction by Plutonium Fallout.

The only additional parameter required beyond those cited above concerning micrograms plutonium per lung cancer dose is the average quantity of plutonium inhaled by humans. In an elegant treatment of this problem, Bennett(3) has provided the estimate that the cumulative inhalation intake through 1972 has been approximately 42 picocuries per person. Since so high a fraction of the total inhaled was inhaled during 1962-1964, and since the years before exceeded the years after, an excellent approximation is that 1962 be taken as an average time of inhalation. Bennett pointed out further that the analysis of tissue burdens suggested the fallout plutonium was most likely to behave like PuO_2, such behavior being what ICRP Task Group on Lung Dynamics would refer to as Class Y compounds (highly insoluble particles).(8)

The calculation of expected number of lung cancers will proceed as in Reference (1), followed by two adjustment factors,

(1) an adjustment for the fact that persons inhaled the plutonium in 1962 versus 1975,

(2) an adjustment (minor in nature) for the retention in bronchopulmonary tissue of the 0.4 micron fallout particles versus those considered in Reference (1).

First Step Calculations.

In Reference 1, the conversion of inhalation to deposition is represented by a factor of four. Therefore, 42 picocuries inhaled represents 10.5 picocuries deposited.

Conversion to picograms of Pu^{239} equivalent yields,

$(10.5)(16.3) \cong 171$ picograms Pu^{239} equivalent deposited.

Lung Cancer Dose, for cigarette smokers, $\cong$ 0.058 micrograms deposited.

, for non-smokers, $\cong$ 7.3 micrograms deposited.

We shall now consider the generation of males in the USA that received the fallout. There was, of course, a spectrum of men, ranging from children through men of advanced age. The treatment of the problem in Reference 1 was for 20-30 year old men. Since the sensitivity of the group under 20 is higher for cancer induction by radiation, and for the group over 30 is lower for cancer induction, a very good approximation is arrived at by considering the entire generation of men to have received the plutonium fallout at the age range 20-30 years. *

Secondly, we shall assume 50% of the men were cigarette smokers; 50%, non-smokers.

At a US population size of $\sim 2 \times 10^8$ people (1962), (approximately ½ men, ½ women), we arrive then at

5×10^7 cigarette smokers (male)
5×10^7 non-smokers (male).

*See Notes 1 and 2 in "Supplemental Notes".

Lung plutonium deposition in each of these groups is

$$(5x10^7)x(171) = 855 \times 10^7 \text{ \underline{pico}grams.}$$

Conversion to micrograms yields

$$(855x10^7)x(10^{-6}) = 8550 \text{ micrograms } Pu^{239} \text{ equivalent deposited per } 5x10^7 \text{ men.}$$

For the smokers,

$$\text{Lung Cancer Doses} = \frac{8550}{0.058} = 147{,}400.$$

For the non-smokers,

$$\text{Lung Cancer Doses} = \frac{8550}{7.3} = 1170.$$

Total Lung Cancer Doses = 147,400 + 1170 = 148,600.

From the definition of the "lung cancer dose", it follows that this calculation means there will occur 148,600 <u>extra</u> lung cancer deaths in the generation of men receiving plutonium fallout.

For women in the population, there are two considerations to make before calculation.

The spontaneous lung cancer rate for women is approximately 0.27 that of men.* While part of that difference may well be accounted for by the difference in cigarette smoking, that is not yet certain, so an intrinsically lower sensitivity will be utilized for women (0.27 x that of men).

Second, we shall divide the female population into 20% cigarette smokers and 80% non-smokers. Therefore,

For $2x10^7$ cigarette smoking women (versus $5x10^7$ smoking men),

$$\text{expected lung cancer doses} = \frac{2x10^7}{5x10^7} \times (0.27)x147{,}400 = 15{,}900.$$

For $8x10^7$ non-smoking women (versus $5x10^7$ non-smoking men),

$$\text{expected lung cancer doses} = \frac{8x10^7}{5x10^7} \times (0.27) \times 1170 = 500.$$

* In the relative risk method (see Reference 1), all radiation effects are calculated as being proportional to the <u>spontaneous</u> occurrence rate of the particular cancer under consideration.

Adding all groups, we have: 148,600 +15,900 + 500 = 165,000 extra lung cancer deaths from weapons-test plutonium fallout, before making the two adjustments described above. These must now be considered.

Adjustment 1:

Since all radiation effects are calculated relative to the spontaneous rates in operation at the time of dosage, we must use the 1962 spontaneous lung cancer fatality rate rather than the 1975 rate of Reference 1.

From the recent American Cancer Society estimates[4] it appears a best estimate is that the spontaneous lung cancer fatality rate for 1962 was $\frac{38}{62.5}$, or 0.61 times as high as for 1975.

Therefore, the first adjustment leads to,

(165,000)x(0.61) = 100,700 extra lung cancer deaths from plutonium fallout.

Adjustment 2:

In the treatment developed in Reference (1), the initial deposition in lung was taken as

8% to tracheobronchial region

25% to pulmonary region.

This led to an estimate that the radiation source to the cancer-relevant cells of the bronchi was 0.18 times as strong as that for the pulmonary region for cigarette smokers.

Bennett recommends, for the 0.4 micron particles of plutonium fallout, that appropriate values are,

8% to tracheobronchial region

32% to pulmonary region.

Correcting the pulmonary region (32% instead of 25%) leads to the relevant bronchial cells having a source 1.15 times stronger; thus, (1.15) (0.18) = (0.207) times that of the pulmonary region.

Therefore, the adjustment factor is 1.15 for this effect. The final adjustment of the expected lung cancer deaths leads to:

(1.15)x(100,700) ≈ 116,000 extra lung cancer deaths in the U.S. population (men + women combined) as a result of weapons-test plutonium fallout.*

This represents the best estimate within the framework of data and assumptions that appearsto deserve use at this time.

Expected Time Distribution of These Extra Lung Cancer Deaths.

When cancer is induced by ionizing radiation, there is a period of time, the so-called latent period, before any extra cancer deaths appear in the exposed population. That latent period is somewhere in the neighborhood of 10-15 years for many types of cancer (only about 5 years for leukemia). Thereafter, the cases of cancer increase until the maximum effect is observed, generally called the "plateau" effect. This plateau may last 30 years, or even the whole remaining lifespan of the exposed population. But it must also be remembered that plutonium (or other radiation) operates as a multiplier of the "spontaneous" (or "natural") occurrence rate of fatal cancers. Most (though not all) cancers show an increasing rate of occurrence with age in a population. Thus, even if radiation doubles the spontaneous rate, at an early period of life the absolute number of cancers occurring will be low. As the exposed population becomes older, the radiation-induced cases will occur in increasingly large absolute numbers. For lung cancer, we can estimate how the radiation-induced fatalities will occur, once the latent period is passed. The Surgeon General's report on Smoking and Health provides the requisite data for estimating the distribution of cases. Using data from that report (p.138)[5], the following tabulation has been prepared, Table 1.

*See Note 4 in Supplemental Notes.

Table 1

Expected Distribution of Lung Cancer Fatalities
by Age Group, After the Latent Period is Over

Age Group	Percent of Ultimate Number of Cases
Under 40 years of Age	0.2%
Under 50 years of Age	2.2%
50-55 years of Age	3.2%
55-60 years of Age	6.8%
60-65 years of Age	11.3%
65-70 years of Age	17.6%
Between 70-80 years of Age	58.8%

In 1975, some 13 years after our "average" time of receiving the plutonium dose, the latent period is just about over, so the lung cancer cases should be starting to occur. However, the largest proportion of the persons who received plutonium fallout were under 35 years of age in 1962. Thus, when these individuals reach 50 years of age, the data of Table 1 suggest that only about 2.2% of the total number of radiation induced lung cancer fatalities will have occurred. So, by approximately 1977, the extra lung cancer fatalities should be (0.022)x(116,000), or 2550 deaths.

The expected rate will then climb fairly rapidly. For example, when the individuals are in the 60-65 year age bracket, the data of Table 1 indicate that 11.3% of the total number of plutonium-induced cancers will occur, and $(0.113)\times(116,000) \approx 13,100$ deaths. Similar calculations can be made for any age bracket. Thus, our existing epidemic of fatal lung cancers will become materially increased from plutonium fallout already received, even if all other factors productive of lung cancer remain constant.

There is a special reason for appreciation of the age distribution of expected cases. In the community of nuclear energy proponents there seems to exist the expectation that all the cases

will occur in a very short time. When the full 116,000 lung cancer deaths don't materialize immediately, we can probably count upon nuclear proponents to say, "See, plutonium isn't all that bad".

The number of weapons-test plutonium-induced lung cancer deaths occurring right now is probably of the order of 1,000 cases per year in the USA, since the latent period is just about over. Over the next couple of decades this number will rise steadily in annual rate. Worldwide, the now-occurring plutonium-induced lung cancer deaths must be of the order of 10,000 cases per year.

Worldwide Lung Cancer Production From Plutonium Fallout.

The plutonium fallout from atmospheric weapons testing is worldwide in scope, with the Northern Hemisphere receiving most of the fallout. While Bennett's calculation of 42 picocuries was derived from New York data, there is no reason to doubt that this is a reasonable approximation worldwide (Northern Hemisphere).

Based upon World Health Statistics[4], the spontaneous lung cancer death rates, age adjusted (1968-69), and averaged over 33 countries of the Northern Hemisphere is 33.3 per 100,000 compared with 44.0 per 100,000 in the USA for the same time period.

Since the relative risk method relates radiation to spontaneous cases, the worldwide (Northern Hemisphere) rate, for plutonium fallout, must be adjusted downward by the factor $\frac{33.3}{44.0}$, or 0.76.

As a first approximation, the Northern Hemisphere population, which received the fallout, was some 10 to 15 times that of the USA. Let us use 10x, to allow for possible differences in fallout received (possibly an underestimate).

Therefore, estimated worldwide (outside USA) cases of fatal lung cancer induced by plutonium fallout is

(116,000)x(0.76)x(10), or 882,000 extra deaths.

Combining USA + outside USA, the total = 998,000 extra deaths.

Probably some 10,000 extra deaths are occurring annually right now.

Life Expectancy Considerations.

There have been some nuclear advocates who have pointed out that radiation-induced cancers tend to occur late in life, say 60 years of age and later, and that the problem is therefore not serious. What these individuals fail to realize is that the life expectancy at 60 years of age, without benefit of plutonium poisoning, is about 15 years. Would the 60 year olds appreciate losing 15 years of life from plutonium-induced lung cancer?

Are The Estimates Consistent With Experience?

There are few specified population samples with known documented exposure to plutonium deposition in the lung. Two exceedingly small groups are known. The first is represented by 25 Manhattan Project workers who had been discovered to excrete plutonium in their urine, and who, as a result, have been under surveillance. Hempelmann and co-workers[6] have reported on the results of such surveillance. The second is represented by 25 workers who received significant lung burdens in the course of the Rocky Flats fire in 1965.

Without any meaningful quantitative approach, a number of observers have suggested that the non-occurrence of lung cancer to date in these two groups means a relatively low lung carcinogenicity for plutonium. Bair, for example,[7] has suggested this. Non-quantitative approaches can lead not only to absurd, irrelevant

conclusions, but also to very serious underestimations of extremely crucial cancer hazards. It behooves us, therefore, to ascertain here whether the experience to date for the Manhattan Project workers or the Rocky Flats workers is or is not consistent with the estimates presented above for the lung cancer of plutonium inhalation.

The Manhattan Project Workers.

At the outset it must be emphasized that the lung inhalation of plutonium by these 25 workers is exceedingly poorly known. This group cannot be treated as in the treatment above, simply because no inhalation data are available. However, some rough estimates can be made for these workers based upon body burdens measured many years after the exposure had occurred. The problem of estimating initial lung deposition from body burden measured 10-27 years after the exposure is severe. Therefore, at best it would be foolish for anyone to base serious conclusions about plutonium carcinogenicity on the tenuous data for these Manhattan Project workers. However, as a rough effort to ascertain order of magnitude consistency with prediction, it is worthwhile to look at this plutonium exposure experience.

There is every reason to consider that inhalation, rather than ingestion, represents the source of the ultimate body burden of the Manhattan Project workers. Thus, if we really knew the body burden, it would be possible to state that originally this burden had been in the bronchopulmonary system. The difficult problems are to know the body burden at a time of decades beyond exposure, to know how to correct this burden back in time (which involves knowing accurately the fraction of plutonium lost from lung via the gastrointestinal tract), and lastly, but extremely importantly, to know the

degree of solubility of the initial plutonium deposited in the lungs. All of these factors are subject to serious error for these workers, which accounts for the statement above concerning the foolishness of serious conclusions based upon the experience of this group of workers.

Hempelmann and co-workers[6] recently reported on several estimates of the "current" body burden, measured at several times, between 1953 and 1972. These authors suggest that their 1972 estimates are probably their best estimates. However, the excretion curve they utilize for periods beyond $\sim$ a few thousand days, based upon relatively short-term measurements of Langham (for periods shorter than 1500 days), are grossly at variance with estimates that the ICRP model suggests for liver and skeleton clearance or that Bennett uses. The nature of the difference is such as to lead Hempelmann and co-workers to overestimate the body burden of these workers by a large factor.

The ICRP model suggests (see Bennett)[3]

For liver, $T_{\frac{1}{2}} \approx 40$ years, for man. (40 years $\approx$ 14,600 days).

For bone, $T_{\frac{1}{2}} \approx 100$ years, for man. (100 years $\approx$ 36,500 days).

Therefore, for liver clearance,

daily elimination fraction $\approx 0.693/14600$, or $\underline{4.7\text{x}10^{-5}}$/day

and, for skeleton clearance,

daily elimination fraction $\approx 0.693/36500$, or $\underline{1.9\text{x}10^{-5}}$/day.

If, as the ICRP model suggests, the liver and skeletal reservoirs are equal in size, then overall excretion would be,

$$\begin{aligned}\text{daily elimination rate} &\approx \tfrac{1}{2}\,(4.7\text{x}10^{-5}) + \tfrac{1}{2}\,(1.9\text{x}10^{-5}) \\ &\approx (2.35 + 0.95) \times 10^{-5} \\ &\approx 3.3 \times 10^{-5}/\text{day}.\end{aligned}$$

The body burden estimates of Hempelmann and co-workers, for their 1972 evaluation (which they prefer) are based upon an excretion fraction at 27 years (9855 days) of $\sim 2.4\times10^{-6}$/day. Their estimate is at variance with what the ICRP model suggests, what ICRP itself suggests[8], and the $T_{\frac{1}{2}}$ values for liver and skeleton calculated above.

The body burden estimated by Hempelmann and co-workers should be reduced by this corrected factor for excretion, which is factor of $\frac{2.4\times10^{-6}}{3.3\times10^{-5}}$, or $\underline{0.073}$.

For the 25 Manhattan Project workers, the 1972 cumulative body burden (all individuals combined) $\doteq$ 2.44 microcuries Pu^{239} equivalent. (per Hempelmann et al).

Applying the correction factor, 0.073, for excretion, We have

Cumulative body burden (1972) $=$ (0.073)(2.44) $\doteq$ 0.178 microcuries.

We presume, since inhalation was the prime route of access for the plutonium, that all this body burden was originally in the lung. But we must allow, additionally, for the loss of plutonium from the lung via the gastrointestinal tract. Of lung deposited plutonium, the ICRP Task Group model suggests:[8]

40% rapidly lost via gastrointestinal tract

40% lost with $T_{\frac{1}{2}} = 500$ days via gastrointestinal tract

20% cleared to (lymph + blood).

Therefore, at times long compared with lung clearance, the body burden should be 1/5 of the <u>initial</u> lung deposit, <u>if</u> the gastrointestinal clearance fraction is correct. Bennett has suggested the ICRP model <u>may</u> overestimate the g.i. tract loss. In any case, use of the factor of 5 to convert from current body burden to

initial lung deposit cannot underestimate the initial lung deposit, since it credits gastrointestinal excretion maximally.

Therefore, conversion of body burden, cumulative, for 25 workers, to initial lung deposit, cumulative, yields

$(5)\times(0.178) = 0.89$ microcuries Pu^{239} equivalent.

In micrograms,

$(0.89)\times(16.3) = 14.5$ μgs Pu^{239} as cumulative initial lung deposit.

The smoking history is not available for these men, so we can assume they may have been comparable with the population-at-large, ½ smokers, ½ non-smokers.

Therefore, 7.25 μgs Pu^{239} is cumulative deposition in smokers

7.25 μgs Pu^{239} is cumulative deposition in non-smokers.

Estimation of Lung Cancer Doses, Cumulative, in Manhattan Project Workers.

Before calculation of expected lung cancer doses in the Manhattan Project workers, there are two adjustment factors required:

(a) Exposure was in 1945. From Vital Statistics data, the spontaneous lung cancer rate in 1945 was 0.22 times that of 1975.

(b) Exposure was, in all probability, to relatively soluble compounds of plutonium, from the nature of the work described for the men. Indeed, Hempelmann and co-workers refer to just 2 of the men as "most likely received exposure to plutonium oxide".

We can, therefore, reasonably assign 90% of the cumulative exposure to Class W compounds; 10% to Class Y compounds.

This would represent an average clearance $T_{\frac{1}{2}}$ of

$(0.9)\ (50) + (0.1)\ (500) = 95$ days.

This would require lung exposures to be corrected by $\frac{95}{500}$, or a factor of (0.19), since all the dosimetry calculations are based upon $T_{\frac{1}{2}} = 500$ days for PuO_2 type aerosols.

Finally, therefore, the lung cancer doses, to be applicable to this group, can be corrected for (a) 1945 exposure, and (b) 90% Class W compounds.

Therefore, for the Manhattan Project workers,

for smokers, lung cancer dose $(0.058) \times (\frac{1}{0.22}) \times (\frac{1}{0.19}) \doteq 1.39$ μgs. Pu^{239}

for non-smokers," " " $(7.3) \times (\frac{1}{0.22}) \times (\frac{1}{0.19}) \doteq 175$ μgs. Pu^{239}

Among the cigarette smokers, cumulative initial lung deposit $\doteq 7.25$ μgs.

so there were $\frac{7.25}{1.39} \doteq 5.2$ lung cancer doses.

Among the non-smokers, cumulative initial lung deposit $\doteq 7.25$ μgs.

so there were $\frac{7.25}{175} \doteq 0.04$ lung cancer doses.

The total, cumulative among the 25 workers, is 5.24 lung cancers, as a <u>lifetime</u> expectation.

Hempelmann and co-workers describe these men as "in their early 50s". Examination of Table 1 indicates that by the early 50s, the men should have developed approximately 3.5% of their lifetime expectation in lung cancers,

or $(0.035) \times (5.24) \cong 0.2$ lung cancer cases.

Since lung cancer cases can't be fractional, we can say there are 4 chances out of 5 that at the "early 50s" we will observe <u>zero</u> cases; 1 chance out of 5 that one case would have been observed.

The <u>observation</u> of zero cases is directly in accord with the calculations above that indicate the very high probability (4/5) of <u>observing</u> zero cases.

Finally, the conclusion is reached that the Manhattan Project experience is totally consistent with the plutonium lung cancer expectations of this report and of Reference 1. No comfort whatever can be drawn from these Manhattan Project experiences concerning any hoped-for lowering of the lung cancer hazard of plutonium inhalation.

The Rocky Flats Workers.

For this group of plutonium-exposed workers the data are much better than for the Manhattan Project workers. First, measurements by body counting were made within a very short period after the inhalation exposure. Second, Mann and Kirchner[9] reported that the exposure was to PuO_2 particles, so we know that Class Y behavior, with a $T\frac{1}{2} = 500$ days for lung clearance, should be applicable.

The data for the individual exposures were recently provided by Rocky Flats Management.* The mean value for the deposition, expressed by Rocky Flats as a time-weighted-average over the 12 months following exposure, for the 25 workers was 31.6 nanocuries, or 0.032 microcuries. This time-weighted average should closely approximate the lung deposition.* The smoking habits of the workers at exposure remains unknown, so we shall approximate this as ½ cigarette smokers, ½ non-smokers. The average age at exposure was 43.6 years.

For 0.032 microcuries, the lung deposition would have been (0.032) (16.3), or 0.51 micrograms per worker. For 25 workers, the aggregate dose = 25x0.51, or 12.8 micrograms of Pu^{239} equivalent.

Therefore, for the cigarette smokers, dose = $\frac{1}{2}$x12.8 = 6.4 micrograms,

for non-smokers, dose = $\frac{1}{2}$x12.8 = 6.4 micrograms.

Estimation of Lung Cancer Doses in the Rocky Flats Workers.

(a) The exposure occurred in 1965. From Vital Statistics data[4], the spontaneous lung cancer death rate in 1965 was 0.69 times that for 1975.

(b) Mann-Kirchner's evidence indicates that the exposure, in all probability, was to PuO_2, so Class Y (insoluble) behavior is expected.

*Supplemental Note(3) provides the individual case data.

For the cigarette smokers,

1 Lung cancer dose is, therefore, $\frac{0.058}{0.69}$, or 0.084 micrograms Pu^{239}

For the non-smokers,

1 Lung cancer dose is, therefore, $\frac{7.3}{0.69}$, or 10.6 micrograms Pu^{239}.

Therefore, for the cigarette-smoking Rocky Flats workers, the lifetime expectation is $\frac{6.4}{0.084}$, or 76.2 lung cancer doses.

For the non-smokers, the lifetime expectation is $\frac{6.4}{10.6}$, or 0.6 lung cancer doses.

Adding these two groups, the lifetime expectation for the Rocky Flats workers is $\sim$77 lung cancer doses, provided the workers were at a mean age of 25 years at exposure. But since the mean age at exposure was 43.6 years, this expectation must be reduced approximately for the lower risk associated with exposure at ages beyond 25 years (see Supplemental Note 1). From Table IV of the Supplemental Note, it is calculated that for exposure at 43.6 years of age, the risk per rad (or rem) is $\frac{1}{4}$ that for exposure at 25 years of age. Therefore, $\frac{1}{4}$ x 77 $\simeq$ 19.3 lung cancer doses as the final corrected lifetime expectation for the Rocky Flats workers.

In order to maximize the expectation, we shall assume that by 1975, ten years after exposure, the latent period for cancer development is over. From Table 1, it is estimated that for men at 53.6 years (43.6 + 10), approximately 3.5% of the lifetime expectation should have occurred.

Therefore (0.035)x(19.3), or 0.68 lung cancers should have occurred. For an expectation of 0.68 cases, the probability is about 0.5 that zero cases will have been observed. And even this is conservative, since the period to reach the full plateau is quite likely to be greater than 10 years. Thus, the non-occurrence of lung

cancers in this small group of workers by 1975 is totally consistent with the lung cancer potential for PuO_2 exposure derived here and in Reference (1). In no way is a lesser carcinogenicity of plutonium suggested by the Rocky Flats experience.

The time to observe the Rocky Flats workers will be in the next five to ten years. These workers did receive exposure to PuO_2 in a respirable particle size and did receive appreciable doses. Their lung cancer death rate some 10 years beyond 1975 will be of great importance. We can hope, for the sake of the workers, that fewer than 50% were cigarette smokers at exposure. Also, since the lung cancer risk is diminished in ex-smokers, it is to be hoped that the workers were advised to cease cigarette smoking after plutonium exposure.

GENERAL DISCUSSION

The calculations presented indicate that at least 998,000 premature lung cancer deaths can be expected to have been irreversibly committed throughout the Northern Hemisphere as a result of plutonium weapons-test fallout.* It is also expected that, worldwide, these must by now be yielding some 10,000 or more lung cancer fatalities per year. But since the lung cancer cases caused by plutonium exposure do not carry any flag that tells us that these particular cases are the ones caused by plutonium exposure, the absurd statement is possible that "I don't know anybody that's died as a result of exposure to plutonium, do you?"(11)

Perhaps biology will evolve, in time, to accomodate the proponents of nuclear energy, by having each cancer sprout a flag indicating each origin. Until that time, we will have to resort to public health science to derive rational understanding of such problems as

ee Note 2 in "Supplemental Notes".

The effort to downgrade plutonium carcinogenicity by pointing to non-occurrence of lung cancers in the small groups of Manhattan Project and Rocky Flats workers is here shown to be a vain effort. The non-occurrence at this early time is in excellent accord with expectations.

It is the documented history of the promotion of nuclear energy that the cancer hazard of radiation has been underestimated on virtually every possible occasion. When the full story became evident with the passage of sufficient time for the radiation-induced cancers to develop, the authoritative bodies responsible for radiation protection have revised their estimates upward. Thus, it was possible for the National Committee on Radiation Protection to state in 1954[12] that 36,000 millirems would be without effect upon humans, while the BEIR Committee in 1972 estimated that 100 millirems per year (3000 millirems in 30 years) might be anticipated to cause 3500 additional cancer deaths per year.[13] (p. 90-BEIR report).

Bair has recently stated,

> "There has been no recorded instance of cancer in man resulting from the internal deposition of any plutonium isotope in the more than three decades that plutonium has been used. The excellent record has resulted from extremely effective control methods."

There is no reasonable framework in which the Bair statement can be defended. It may even be supposed that Bair may wish to reject all the calculations of this report and of Reference 1. In that event, Bair would be forced to examine his own published data on lung cancer induction by PuO_2 in the beagle dog. The maximum difference between his beagle data and these calculations for humans is a factor of 3.7 fold.[1] Therefore, instead of 998,000 lung cancer

fatalities irreversibly committed by plutonium exposure, Bair would have to estimate at least 270000 fatal lung cancers irreversibly committed. This is a long way from the suggestion above of no cancers from plutonium exposure.

Bair would be correct that the plutonium-induced cancers are not "recorded". But that is only because human cancers have not evolved to the point of printing out a label indicating which of the various carcinogens caused the particular case in point.

Some Implications of the Lung Cancer-Plutonium Fallout Estimates for the Developing Nuclear Power Industry.

The current estimates indicate the number of fatal lung cancers produced for a known fallout intensity. It becomes possible, therefore, to estimate, for various degrees of containment achieved, what the expected number of lung cancers will be from the nuclear power industry. It cannot be assured that the nature of fallout particles from releases in the nuclear power industry will be identical with that for weapons testing. The situation could be worse, equal, or better. The best estimate, within current knowledge, is that the fallout will be similar in character. The calculations will proceed from an estimate of the amount of weapons-test plutonium fallout over the USA to an estimate of the amount, in comparison, that would fall out at various levels of containment in the nuclear power industry. The lung cancer consequences are then directly available by comparison with the results of this report for weapons-test plutonium fallout.

A first approximation to the total plutonium deposition in the 50 states of the USA can be obtained from Bennett's data for New York.[3] His estimate is that the cumulative deposition through 1972 is 2.65 millicuries per km^2 for the New York area. Assuming the average

deposition for the USA is not far different from that for New York, this means that for the USA, with an area (including Alaska + Hawaii) of 3.62×10^6 mi^2, or 9.27×10^6 km^2, the total deposition was (9.27×10^6) $(2.65) = 2.46 \times 10^7$ millicuries, or 2.46×10^4 curies Pu^{239} equivalent. Conversion to grams yield (2.46×10^4) $(16) =$ 3.94×10^5 gms. Conversion to pounds yields $\frac{3.94 \times 10^5}{454}$, or $0.87 \times 10^3 =$ 870 pounds. So, approximately 900 lbs. of plutonium were deposited in the USA through 1972 from weapons testing.

The Tamplin-Cochran estimate (see Reference 1) is that the developing nuclear power industry, from AEC projections, will involve the handling of 400 million pounds for plants installed through the year 2020. Since this will be reactor-grade plutonium, it will be approximately 5 times as α-active as the weapons grade plutonium. Therefore all cancer estimates must be multiplied by five-fold to correct for reactor-Pu versus Pu^{239}.

In the calculations presented here, the deposition of 900 pounds of weapons plutonium has committed some 116,000 lung cancers for the USA. It is instructive to ask what various levels of containment in the nuclear power industry imply for the future production of lung cancers. For such an estimate, it will be <u>assumed</u> that the inhaled plutonium per pound of Pu dispersed will be comparable to that for weapons fallout. In fact, it may turn out to be equal to, greater or less than the case for weapons fallout.

Containment Perfection	Pounds Pu Dispersed	Lung Cancers Produced (corrected for reactor grade Pu)
99%	4,000,000	2,575,000,000
99.9%	400,000	257,500,000
99.99%	40,000	25,750,000
99.999%	4,000	2,575,000
99.9999%	400	257,500
99.99999%	40	25,750

Considering the fallibility of men and equipment plus circumstances of accidents, it would hardly be surprising that containment will not be better than 99.99%, and that represents excellent containment under industrial circumstances. The lung cancer production would be, for such excellent containment, a total of some 25,750,000 cases. Since these cases would be spread over about 50 years, it would represent 500,000 additional lung cancer fatalities per year. Since the current death rate from all causes combined in the USA is about 2,000,000 per year, a nuclear-based energy economy with 99.99% perfection in plutonium containment could mean a 25% annual increase in total death rate from this one source alone. The prospects seem hardly less gloomy even for 99.999% perfection in containment, a containment level that falls squarely in the miracle realm.

It is to be noted that the assumption being made here is that under the circumstances of plutonium release from the nuclear power industry, the plutonium dispersal would be limited to USA, rather than worldwide.

Supplemental Notes

Note 1: Sensitivity to induction of cancer by ionizing radiation is age-dependent. The following table (excerpted from Reference 10) describes the sensitivity variation quantitatively.

Table IV (from Reference 10)

VARIATION IN CANCER INDUCTION PER RAD WITH AGE

These estimates represent a step function approximation in reasonable accord with the data points available in the text.

Age at irradiation (years)	Increase in cancer mortality rate per rad (in Plateau Region) (per cent)
In utero	50
0-5	10
6-10	8
11-15	6
16-20	4
21-30	2
31-40	1
41-50	0.5
51-60	0.25
61 and beyond	Assumed negligible

Note 2: It has been stated here and in Reference 1 that the period on the plateau of radiation effects may be 30 years or it may be the entire lifespan of the exposed population. It must be pointed out that *if* the plateau truly lasts only 30 years, then the estimated number of lung cancer deaths from inhalation of weapons-test plutonium fallout would require revision, most probably in a downward direction. Crudely, this would be so because for those individuals exposed early in life, e.g. below 20 years of age, the 30-year plateau period (after the latent period) could be over *before* these individuals have reached the ages characterized by high *absolute* lung cancer fatality rates.

A more refined treatment would also require consideration of the additional fact that for those exposed while very young, the cigarette smoking factor is almost certainly absent, so that there would be a revision required in the lung cancer dose for such individuals. Such a refined treatment, similar to that of Reference 10, would divide the population exposed by age decade at time of exposure, would calculate an appropriate lung cancer dose for each age decade, and would calculate the absolute numbers of expected fatalities for various plateau durations, particularly for 30 years and for the remaining lifespan of the exposed populations.

The currently-presented calculations really represent a hybrid calculation. They tend to underestimate the overall effect by crediting only 30 years as the period at risk. On the other hand, for the reasons stated above relating to expiration of the plateau period, they tend to overestimate the overall number of cancers. The refined calculations will be presented in a later report of this series. It must be emphasized, however, that ultimately the real resolution to the problem must come from determination of plateau duration in humans through continued followup of exposed population groups, e.g., the Hiroshima-Nagasaki and spondylitis groups.

Note 3: The individual exposure data for the 25 Rocky Flats workers are not recorded in the published literature, nor are their ages. Since the Rocky Flats Management was exceedingly cooperative in providing these data, they are reproduced as Table V below.

The immediate lung deposition in these workers versus time-weighted average (as in Table V) would depend critically upon the

exact time after exposure for each initial measurement and on the very early clearance fraction of deposited PuO_2 in man. Since these are not available, there is no way to correct the data here for these effects. At most, the lung cancer expectation would not be increased by a factor of two, so that no change in conclusions reached would be indicated. And since the expectation has been maximized by assumption of full plateau by 10 years, the argument presented is further strengthened.

Note 4: It is highly probable that the bulk of the exposure reflected in Bennett's inhalation estimates are from direct fallout of plutonium rather than from resuspension of already deposited plutonium. Estimation of contributions from resuspension is difficult, during a period when direct fallout is still occurring. To the extent that resuspension occurs in the future, the estimated numbers of lung cancers will increase beyond the estimates presented here.

Note 5: In the discussion of the Manhattan Project and Rocky Flats workers, the possibility of having more lung cancer doses than the number of workers was included. It is self-evident that it only takes one cancer to kill a person. However, it is essential to allow for multiple lung cancer doses per person for correct analysis. In actual observation, effects arising from this are manifested as an earlier appearance of the lung cancers that would be otherwise expected.

TABLE V

DOW

THE DOW CHEMICAL COMPANY

ROCKY FLATS DIVISION
P. O. BOX 888
GOLDEN, COLORADO 80401

June 23, 1975

John W. Gofman, M.D.

RESPONSE TO REQUEST FOR INFORMATION ON 25 EMPLOYEES
EXPOSED TO PLUTONIUM IN OCTOBER 1965

The following is a list of employees by age and their respective plutonium exposures. The amount in the lungs (chests) of the 25 employees is a time-weighted-average* over the 12 months following the exposure.

age	plutonium (nCi)	age	plutonium (nCi)
24	13	44	7
24	16	45	12
24	19	46	11
29	15	49	20
33	56	52	100
33	12	53	140
38	8	56	130
39	14	56	12
39	23	59	34
39	18	59	59
40	18	60	10
42	9	64	24
42	11		

* Time-weighted-average of 16 nCi in the lung produces 15 REM per year.

We have no records of individual's smoking habits.

H. E. Bowman
H. E. Bowman
General Manager

CRL:mk

cc: W. M. Lamb, RFAO
C. R. Lagerquist

LIFE IS FRAGILE · HANDLE WITH CARE

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Testimony for the GESMO Hearings

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Committee for Nuclear Responsibility, Inc.

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Testimony by John W. Gofman for the NRC GESMO Hearings

The testimony presented here addresses the considerations of the toxicity of plutonium. It addresses GESMO, Volume 3, Section lV-J, Radiological Health Assessment. There are other sections of GESMO which also address or mention the toxicity of plutonium. To the extent that this is so, this testimony addresses those sections as well.

Contents of Testimony

Note: All five items listed above are integral parts of this testimony.

General Response to All Critiques

of

John W. Gofman's Estimates of the Lung Cancer Hazard of Plutonium

Introduction:

The estimates of the lung cancer hazard of inhaled plutonium are set forth in three papers by John W. Gofman. These are

(1) The Cancer Hazard From Inhaled Plutonium, CNR Report 1975-1R, dated May 14, 1975

(2) Estimated Production of Human Lung Cancers By Plutonium from Worldwide Fallout, CNR Report 1975-2, dated July 10, 1975

(3) The Plutonium Controversy, JAMA, 236, No.3, pp284-286, July 19,1976.

For simplicity in the ensuing discussion , reference to these reports of a general character will be by the term "Gofman-Pu papers". References of a specific character will be to "CNR-1R", or "CNR-2", or "JAMA".

Before the appearance of the Gofman-Pu papers, the general "wisdom" was that the lung cancer potential of plutonium could be adequately described by consideration of the deposition and retention of PuO_2 aerosols (or other insoluble Pu compounds as aerosols) in the lung tissue of the bronchiolo-alveolar regions, with neglect of any potential effect on more proximal bronchial tissues, that is tissues proximal to the terminal bronchioles. The models used in achieving that "wisdom" predicted no significant long-term retention of plutonium would occur in the bronchial tissues proximal to the terminal bronchioles. Hence it was predicted that there would be no significant dose to such more proximal bronchi. Gofman raised the question as to whether the conventional "wisdom" might be in error, which it certainly seems to be. Since even a small percentage retention in the proximal bronchial tissues could drastically increase the lung cancer hazard of plutonium aerosols, it becomes of extreme importance to know just how much plutonium is subject to long-term retention in the bronchial tissues proximal to the bronchiolo-alveolar region. The plain fact is that no studies have been accomplished to describe such retention of plutonium in either of two major classes of humans, cigarette smokers and non-smokers. It is regrettable that such studies have not been accomplished .It is further an indication of the lack of competence of AEC and ERDA that in all their highly-funded studies of plutonium, these crucial questions have not been addressed, particularly when one considers the central importance of the issues in-

volved.

In the Gofman-Pu papers it was pointed out that the major part of the mechanism counted upon to remove Pu rapidly from the bronchi was the set of cilia lining the bronchial cells. These cilia beat in a direction such that material is propelled in the direction of the nasopharynx. The actual material being propelled is, at least in part, mucus secreted by goblet cells in the bronchial lining, such mucus probably incorporating part of the particulate material being cleared. Part of the clearance mechanism may also be the propulsion of cells of the macrophage class by cilia. The cilia are severely damaged in humans who smoke cigarettes. Such damage is two-fold in character, namely actual denuding of the cilia of cells and functional impairment of ciliary action in those retaining cilia. Additionally the smokers have replacement of the normal bronchial epithelium by metaplastic epithelium in numerous regions. Similar alterations occur in non-smokers but to a far lesser degree. JAMA presents the references both to the studies of Ide and of Auerbach on these issues.

No one really knows why PuO_2 aerosols are retained so tenaciously in the bronchiolo-alveolar region, tenaciously enough to give $T_{1/2}$ clearance times of the order of 500 days from this region. But there is no doubt that the clearance time is long. Many who have commented on the long-term retention in this region have ascribed it to the absence of ciliated cells, so that the so-called "muco-ciliary escalator" doesn't work there as it does in the more proximal bronchi.

If the cilia are absent in 25-30 % of the bronchial cells and if the cells are altered to metaplastic epithelium, what has happened in such regions to the ability to clear PuO_2 aerosols? As Gofman pointed out carefully in the Pu papers, there simply are no data on humans (nor any valid experimental animal model) to answer this crucial question. No data at all. Gofman therefore suggested that, as a first approximation, the best estimate is that clearance from such regions of ciliary absence and metaplasia will become equivalent to that for the non-ciliated bronchiolo-alveolar region , yielding $T_{1/2}$ values of the order of 500 days. To be sure, altered bronchial epithelium is not identical with bronchioloalveolar tissue. It may be, as Gofman pointed out, that altered bronchial epithelium is more capable, equally capable , or less capable in PuO_2 clearance compared with bronchiolo-alveolar epithelium. It is the height of public health irresponsibility to assume that clearance is unaltered in the damaged

bronchial epithelium. Such an assumption emanating from laboratories supported by ERDA or NRC should be totally suspect, in view of the vested interest of those laboratories in a low order of plutonium toxicity.

The Criticisms of the Gofman Hypothesis

In five separate reports, the ERDA laboratories have produced critiques of the Gofman-Pu papers. While I shall deal with each of those reports separately below, the substance of the criticism has to do with two points:

(a) The $T_{1/2}$ clearance time utilized by Gofman for the injured bronchial epitheium just "must be too high", and

(b) Gofman has used a much higher figure for the relative risk of lung cancer per rem of ionizing radiation than the BEIR committee and this accounts in part for his high toxicity of plutonium.

I shall deal with these two points in this general response. As for the remainder of the criticisms, they are so far over on the side of absurdity that they hardly deserve any comment whatever. This will be demonstrated in the responses to the specific critiques.

Criticism (a)

Other than wishful thinking, the five ERDA lab critiques have provided nothing at all to challenge the Gofman value of $T_{1/2}$ of clearance from injured bronchial epithelium. They have, therefore, assumed (not presenting any basis whatever) that even after cilia have been lost and damaged, after normal cell epithelium is replaced by metaplastic epithelium, this bronchial epithelium will still be far more efficient in clearing PuO_2 particulates than the non-ciliated epithelium of the bronchioalveolar region. In the absence of any evidence to support their position, they simply choose to be optimistic about the lung cancer potential of plutonium . This is public health irresponsibility at its worst! To the extent that ERDA and/or NRC accept(s) such nonsense as science , they are being totally derelict in their legal responsibilities concerning the health and safety of the public. All estimates of the hazard of the nuclear fuel cycle, of reprocessing, of mixed oxide fuel fabrication, of appropriate standards for so-called " permissible" exposure either to the public or to occupational workers are simply worthless until the question of plutonium retention in the bronchial epithelium is resolved. Gofman

has made the very reasonable (and modestly conservative) suggestion that conversion of ciliated bronchial epithelium to non-ciliated epithelium plus some metaplastic epithelium makes them act , with respect to PuO_2 retention, like non-ciliated bronchiolo-alveolar epithelium. Moreover, he suggested that metaplastic epithelium might be more active, by phagocytosis or endocytosis, than is the case for the bronchiolo-alveolar region, and that this could indeed make his estimate inadequately conservative. Gofman emphasized that the burden of proof that his proposed behavior does not represent reality rests upon those who choose to believe a more "optimistic" behavior will occur. This is clearly and unequivocally as it should be in such matters of public health importance, a point not apparently appreciated in any of the five ERDA lab critiques. While such lack of appreciation cannot be condoned, it can be understood in the light of the desire not to bite the hand that feeds them.

Meanwhile, in striking contrast to the opportunistic gobbledygook presented in the five ERDA lab critiques, there has appeared a most valuable study of human material by Radford and Martell. This paper bears directly upon the clearance of insoluble particulates from bronchial tissue. The paper is " Polonium-210: Lead-210 ratios As An Index of Residence Times of Insoluble Particles From Cigarette Smoke in Bronchial Epithelium" by Edward P. Radford and Edward A. Martell, and was presented at The Fourth International Symposium on Inhaled Particles and Vapors , Edinburgh, 22-26 September 1975. (in press, Pergamon Press Ltd.). The introduction to this paper, appearing just a few months after the CNR-1 and CNR-2 papers, reiterates and confirms the points made by Gofman, and it does that so succinctly, that I think it important to reproduce that introduction here as follows: (I now quote Radford and Martell)

> " Up to the present, models of the deposition, retention, and clearance of insoluble particles from pulmonary tissues have emphasized processes within the lung parenchyma, with the bronchi being considered primarily conduits through which mucociliary clearance occurs. There is recognition that preferential deposition of some inhaled particles occurs by impaction at special locations in the bronchial tree, such as at bifurcations of the trachea and major bronchi, and there is also evidence that localized regions of the epithelium may have inefficient ciliary clearance because of splitting of the mucociliary stream at bifurcations, development of squamous metaplasia and loss of ciliary function in small areas, or other mechanisms affecting mucociliary competence. For these reasons, one would conclude that in some regions

(Radford-Martell quote continued)

of the bronchial epithelium, especially at bifurcations, localized concentration of insoluble inhaled particles might accumulate, both by direct deposition or after entrainment in the mucociliary stream and subsequently becoming trapped in regions of of inefficient clearance. Especially in relation to development of bronchial cancer, certainly one of the most serious consequences of inhalation of particles, the failure of current lung clearance models to take account of the retention of materials in the bronchial epithelial structures is one of the most serious limitations of these models."

This quotation expresses precisely the same type of thinking presented in the Gofman-Pu papers.. The experimental data of Radford and Martell are used to try to estimate the residence time (mean value) for the Lead-210-containing insoluble particles in mainstream cigarette smoke. These authors point out that the presence of some soluble Lead-210 in bronchial tissues will have the effect of giving too low a mean residence time for the insoluble particles by their method of analysis. Yet, in spite of this, they estimate a mean residence time of 3 to 5 months for the insoluble particles. This , in days , is between 90 and 150 days. Taking a middle value of 120 days this would be some six times lower than the mean residence time Gofman estimated for PuO_2 aerosols in the injured part of the bronchial epithelium (500/0.693 is approximately 700 days). It must be noted that the Radford-Martell values are for overall bronchial tissue samples, not the injured regions alone. Thus over and above the Lead-210 (soluble) error which tends to make their residence times too low they are including some normal bronchial tissue which may have a very much shorter residence time than the injured tissue. When these two factors are taken into account, their experimental evidence on human material may finally suggest an even longer $T_{\frac{1}{2}}$ for injured bronchial tissue than that suggested by Gofman. These data ,preliminary as they are, are enormously closer to the estimates of Gofman than to the optimistic suggestions of a fraction of a day or a day for $T_{\frac{1}{2}}$ in injured bronchial epithelium to be found in the ERDA lab critiques.

Dr. J. Martin Brown (Health, Safety , and Social Issues of Nuclear Power and the Nuclear Initiative, Chapter 4 in "The California Nuclear Initiative" , Stanford University Institute for Energy Studies, April 1976) stated, concerning the ERDA critiques, the following;

" However, none of the critics seems to be aware of recent data

showing the extensive holdup of insoluble particles of polonium (an alpha-emitter, like plutonium , that is concentrated on tobacco leaf hairs) in the bronchial tree of smokers. This, at least qualitatively, supports Gofman's hypothesis and certainly suggests further study is urgently needed."

Dr. Brown is referring to the Radford-Martell studies cited above.

Of all the five ERDA lab critiques, only one, that by Healy and co-workers,shows even a glimmer of appreciation of the problem of plutonium retention in the bronchi. In their conclusion they state

> " In our review of his papers we have concluded that the speculations of Gofman require the arbitrary acceptance of too many numerical parameters and unconfirmed mechanisms to be acceptable as even an approximate numerical estimate of potential lung carcinogenesis by plutonium . There is, indeed, a paucity of direct measurements of clearance rates for intact and damaged bronchial ciliated epithelium but current information would indicate that the problem is not as serious as postulated by Gofman. We would recommend that measurements continue with more emphasis on the absolute bronchial retention, and that until such evidence is available , the Gofman predictions be regarded as interesting and imaginative speculations which should serve to stimulate increased interest in certain phases of current studies. However, we cannot concur with his often statedposition that speculation, no matter how poorly founded, is a proper basis for public health decisions."

It is of some importance to dissect this amazing paragraph of admission of ignorance concerning the clearance of plutonium by intact and damaged epithelium. Starting with the gratuitous insult at the end of their quote, I might point out that it will rain 40 inches per day in the Sahara Desert before the Healy group will be able to show a <u>single</u> instance where Gofman suggested that speculation , no matter how poorly founded , is a proper basis for public health decisions , let alone " his often stated position" . Nevertheless, one must be tolerant in such matters. Here is one of ERDA's set of experts on plutonium toxicity publicly admitting that there is absolutely no way that they can help refute Gofman's estimates, admitting that Gofman is correct concerning the absence of the crucial measurements, advising that the measurements Gofman says are essential must be given high priority. It is easy to understand their frustrations about their admissions which lead them to use insults when they have no science to offer in refutation.

There is one additional point in the Healy et al critique that deserves translation. That is in the quote above where they say "current

information would indicate the problem is not as serious as postulated by Gofman". On a matter of such transcendent importance one would expect scientists, in possession of such "current information", would tell us how serious the problem is, if it is not as serious as postulated by Gofman. On this, they are silent. One cannot help thinking, since they do not provide the " current information", that what they mean is current wishful thinking that might please ERDA.

Criticism (b):

The essence of this second criticism is that Gofman used too high a value for the relative risk of lung cancer development per rem of exposure . Cited as evidence that Gofman used too high a value are a value of 0.20 for the relative risk and a value of 0.29 for the relative risk both in the BEIR Report. I shall return to the excellent reasons why Gofman rejected these values. At this point I should like to take up the reason why Gofman quoted the BEIR statement that the relative risk might be 0.5% or even higher--- and even so Gofman rejected that value as four times too low. Much of this discussion is in a sense moot since new evidence is now available from the Hiroshima-Nagasaki studies (1975) and they not only show that Gofman's choice of 2% per rem was the proper choice at the time, but also the new evidence indicates that even the Gofman estimate of relative risk per rem might be too low, NOT too high.

The relative risk method suggests that radiation acts as a multiplier on the risk of carcinogenesis from so-called "spontaneous" or "natural" causes. Now, virtually everyone in the cancer field considers that the so-called "spontaneous" causes of cancer are ,at least to the extent of some 90% of all causes, environmental factors of a variety of sorts. In the case of lung cancer in man, it is widely accepted that the environmental factor is the smoking of cigarettes, and it is widely considered that cigarette smoking is responsible for 90 % of all human lung cancers. To my knowledge the only contrary opinion is that of the tobacco industry, which is hardly the surprise of the century. If radiation acts as a multiplier of "spontaneous" cancer, the corollary is that it will, in general, act as a multiplier of the cancers caused by the environmental factors that are the basis for so-called "spontaneous" cancers. Is this reasonable? Particularly in the case of lung cancer induced by alpha particle radiation which

is at issue here, is this reasonable? The answer is overwhelmingly in the affirmative. An excellent study was reported by Lundin et al

(Lundin, F.E., Jr., Wagoner, J.K., Archer, V.E. "Radon Daughter Exposure and Respiratory Cancer: Quantitative and Temporal Aspects" NIOSH-NIEHS Joint Monograph No.1 US DHEW, 1971.)

contrasting the effect of alpha particle irradiation of uranium miners in the production of lung cancer in cigarette smokers versus non-smokers . They demonstrated that the effect of radon daughter alpha particle exposure was approximately ten times higher in the cigarette smokers than in the non-smokers for production of human lung cancer. It would take the utmost mayhem with these results to interpret the radiation as anything but a multiplier. These results are very directly relevant since they arise from high LET radiation (alpha particles) which is at issue with respect to plutonium radiation. But there have long existed data concerning radiation as a multiplier for low LET radiation as well. The studies of Stewart and her colleagues on the effect of pre-natal radiation (X-rays) in induction of cancer and leukemia in childhood are by now classic. A prime result of those studies was the demonstration that cancers and leukemias were induced by radiation in proportion to the "spontaneous" occurrence of those diseases.

Among those who fail to understand this problem there seems to exist the mystical idea that the relative risk method and the absolute risk method give rise to different answers concerning the number of cancers per rem of ionizing radiation. Nothing could be further from the truth. Both methods must give the same answer or one of the methods is simply not correct! The virtue of the relative risk method is that it enables predictions in situations where actual experimental data are unavailable. Should the results of the relative risk method ever predict any result at variance with absolute risks as measured, one would have to give up the relative risk method. That has not yet happened in any study with reliable, meaningful data.

A crucial feature of the relative risk method is its prediction that the multiplier for radiation is very sensitive to the age at irradiation. In CNR-2, Gofman reproduced a table (Table lV from Reference 10 cited there) showing how the percent increase in cancer mortality per rad varies with age. For the 21-30 year age group (and it is this age group that CNR-1, CNR-2 and JAMA explicitly address) the value is 2 % per rad. For the 41-50 year age group the value drops to

0.5 % per rad. Gofman and Tamplin published a report in 1970

Gofman, J.W. and Tamplin, A.R. " Radiation-Induction of Human Lung Cancer", Hearings of the Joint Committee on Atomic Energy, January 28, 1970) pp. 389-399.

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which presented the doubling dose for radiation induction of human lung cancer based upon the Japanese studies of Wanebo et al . That doubling dose was estimated to be 79.7 rads and applied to a population of Hiroshima-Nagasaki subjects over 35 years of age. A value of 79.7 rads as a doubling dose corresponds to 1.25 % per rad. Just recently the extended data from Japan have appeared in a paper by Beebe and Kato from which the per cent increase in cancer per rad

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Beebe, G.W. and Kato, H. "Review of Thirty Years Study of Hiroshima and Nagasaki Atomic Bomb Survivors II. Biological Effects,E. Cancers Other Than Leukemia",Journal of Radiation Research , Supplement, pp. 97-107, 1975.

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for lung cancer can be estimated. The data of Table 1(reference just cited) give a relative risk of 140% for the group having received between 10 rads and 100 rads and a relative risk of of 200% for the group having received over 100 rads. The mean dose for the 10-100 rad group cannot be far different from 50 rads. It would be a <u>conservative</u> estimate to assign a value of 200 rads for the group over 100 rads . These data then translate into 0.8% per rad for the 50 rad mean, and into 0.5% per rad for the 200 rad mean. Since there is no overt reason to weight one group more than the other, the final best estimate is 0.65 % per rad. It is virtually certain that this value is too low, though we shall not alter it. The reason it is too low is that all cancers developing between 1950 and 1972 are included. This means that part of the observation period was during the latent period for lung cancer development. Thus spontaneous cancers occur during the latent period, but radiation-induced ones do not, giving a falsely low value for the radiation-induced cancers relative to the spontaneous ones. The earlier estimate of Gofman-Tamplin of 1.25 % per rad is probably closer to the true value, but since it is an estimate of the effect of including part of the latent period , we shall not insist on it here, and shall accept the 0.65% per rad figure that comes out of the extensive recent Japanese data. Beebe and Kato are quite explicit in stating that these data refer to those who were 35 years of age or older at the time of bombing. They do not provide

the mean age at time of bombing for this group , but that mean age cannot possibly have been far away from 45 years. If we use 45 years we shall be conservative. So, for a group of mean age of 45 years we have a mean value of 0.65% increase in cancer per rad, versus a value of 0.5% predicted by Gofman and Tamplin through the relative risk method. Calculating back to persons 21-30 years of age, the new Japanese data would suggest 2.6% per rad rather than the 2 % per rad utilized by Gofman in the Gofman-Pu papers. <u>If anything ,therefore, Gofman underestimated the cancer hazard of radiation</u> .

The ERDA lab critiques suggest that Gofman should have used a value of 0.2 % per rad (actually 0.19%) to be found in table f-2 of the BEIR report. This suggestion is so absurd on the face of it that it shouldn't require explanation, but apparently explain we must. First of all the BEIR value is for people <u>over</u> 35 years of age* Actually the BEIR committee erred in that table because they label the group as having a <u>mean</u> age of 35 years* Beebe and Kato are explicit in pointing out that even now they are unable to address the issue of persons under 35 years* The most probable estimate of true mean age for the group was 45 years or more. Since the Gofman-Pu papers address the risk for persons 21-30 years(and most explicitly so), it would be appropriate to multiply that 0.2% value by four, giving 0.8%. But this is only the beginning of the error that would occur using the crude tabulated value from BEIR. The BEIR Committee recognized this in a very important paragraph which is here reproduced from p.156 of the BEIR report;

> " All four of these groups are still under investigation, and it is probable that because of the long latent period for lung cancer, the rates calculated will rise as further cases develop. This is particularly true for the spondylitis patients. It is possible , therefore, that in the final analysis the absolute risk in these groups will approach $2/10^6$/year/rem and the relative risk reach 0.5% or higher. For the three groups (miners and Japanese survivors in which up-to-date information is available,)it is significant that many new cases have been added in the past few years."

Note that even the 0.5% figure suggested would have to be corrected to convert the value to that appropriate for 21-30 years of age. So, the BEIR Committee very explicitly tells us that the value in the table is outdated, is contaminated by cases in the latent period, and that hence that value may be seriously low. The BEIR Committee provides some guidance as to how high the value might go. All Gofman

Values with a star (*) refer to age at time of bombing.

did was to read what the BEIR Committee said about its own values. The ERDA lab critiques would suggest that reading is out of style.

There is no rational way that the criticism that Gofman used too high a value for relative risk per rad will stand up. If we add to this the fact that the most recent extensive Japanese data suggest Gofman may have underestimated the value for relative risk per rad, it would appear that this ERDA Lab criticism falls of its own weight unequivocally.

Lastly on the issue of estimation of risk per rad, some of the ERDA lab critiques have exhumed the Neanderthal argument that the linear hypothesis should not have been used by Gofman in the Gofman-Pu papers. There is every reason to have used the linear hypothesis in estimating the lung cancer hazard of plutonium. First of all, it represents public health responsibility, instead of irresponsibility as has been pointed out by virtually every serious group concerned with radiation protection. That the National Council on Radiation Protection and Measurements might not concur is hardly surprising for this is the same nuclear industry hack organization that in 1954 stated that 36000 millirems would be without physical effect on man. The Environmental Protection Agency , in its 1976 rule-making on radiation standards , has just rejected the NCRP suggestions. But public health responsibility is only one reason why the linear hypothesis should be used . Let us consider the evidence abundantly available to us.

1. Referring to the Spiess- Mays studies(Spiess,H, Mays, C.W., "Bone Cancers Induced by ^{224}Ra(ThX) in Children and Adults", Health Physics, Vol. 19, Dec. 1970, pp 713-729) the BEIR Committee stated the following;

> " The data for the ^{224}Ra-injected patients are consistent with the linear nonthreshold dose-response curve within the limits of the dose range available and when the dose is expressed as mean dose to bone."

It is worth noting that these studies are for high LET alpha radiation in humans, the same type of radiation as for plutonium.

2. The data on Ra^{226} induction of bone cancer in man have been re-analyzed by several workers after Evans claimed that those data were inconsistent with the linear hypothesis. One such refutation is by Gofman,J.W. and Tamplin, A.R. " The Question of Safe Radiation

Thresholds for Alpha Emitting Bone Seekers in Man ", Health Physics, Vol. 21, July, 1971, pp 47-51. Again, these are data for high LET alpha radiation in humans and they are consistent with the linear non-threshold hypothesis.

3. In an analysis of the induction of lung cancer in the uranium miners, Gofman and Tamplin showed that the doubling dose in working-level-months for radon-daughter induction of lung cancer showed no evidence of increasing at the low dose ranges, a fact supportive of the linear, non-threshold hypothesis. Indeed, in that analysis what variation in doubling dose occurred suggested, if anything, that the radiation effect might be even higher than predicted by linear thesis for the low-dose region. (Gofman, J.W. and Tamplin,A.R. , "The Colorado Plateau: Joachimstahl Revisited : An Analysis of the Lung Cancer Problem in Uranium and Hardrock Miners" Testimony presented at the Hearing of the Joint Committee on Atomic Energy, 91st Congress of the United States, Jan. 28, 1970, and GT-106-70.)

4. E.E. Pochin, in a very recent paper (Pochin, E.E, "Radiology Now: Malignancies Following Low Radiation Exposures in Man" British Journal of Radiology, Vol.49, No. 583, 577-579, July 1976) has commented that for the low LET radiation (X-rays) exposure of infants-in-utero, the risk of malignancy is linearly related to dose. The direct quote of Pochin follows;

> " The inference that the radiation caused malignancies was, however, questioned at first. For example, a sub-group of mothers might have been more frequently X-rayed than mothers in the control group because of hereditary abnormalities, e.g. of the pelvis, and this sub-group might have a preponderance of children who developed malignant disease as a result of some associated congenital abnormality.
>
> Such a source of bias now appears to be excluded, however, by two considerations. Firstly, Stewart and Kneale(1970) showed that the likelihood of subsequent malignancy per examination increased about linearly with the number of films used in the examination, where this number was known. ....."

Linearity , for these studies, covers the range of about 0 to a few rads, and for low LET radiation at that.

It appears that the public health problem of radiation protection will not indefinitely be burdened by the old crowd of AEC diehards, now wearing ERDA gowns. There are, after all, retirement rules and time has a way of marching on.

Conclusions:

The critiques of the Gofman-Pu papers have been considered here in detail, with respect to the points deserving serious comment and those critiques are rejected in toto. Some very trivial criticisms from the five ERDA Lab critiques will be dealt with in the Appendix to this testimony. The only reason to deal with them at all is to re-assure the reader that major gems do not exist there which cannot be answered.

I therefore suggest that the NRC in its GESMO considerations accept the estimates of plutonium lung cancer hazard precisely as they appear in the three Gofman papers which are part of this testimony, CNR-1, CNR-2, and JAMA. It is my considered opinion the GESMO must deal with the consequences of those estimates if the NRC chooses to be responsible in its mission.

Future evidence may show that the Gofman estimates are correct as they stand, are too high, or are too low, as Gofman has clearly and explicitly stated in those reports. And if and when such evidence(not wishful thinking) appears, the estimates can be appropriately altered, if necessary. There does exist one possible source, aside from the issue of $T_{\frac{1}{2}}$ of clearance,that could force some downward revision of the lung cancer hazard of plutonium from that estimated by Gofman. If some of the energy of alpha particle decay is expended in cells other than those at risk for carcinogenesis or is expended in mucus , the true dose to the cells at risk would have been overestimated. But we simply do not know the physical distribution of the plutonium that is retained long-term, so there is no way now to estimate whether a correction is needed or how large it might be. A correction of the order of two-fold is by no means inconceivable. On the other hand there is also the possibility that the RBE factor should have been chosen as 20 instead of 10, which would introduce a factor of two in the opposite direction. Probably other minor factors will occur to alter the estimates. This is why Gofman suggested that the final result might be a factor of two or three too high or too low (JAMA, p285). The key point is to realize that any correction required could go either way, and hence from the public health point of view there is no basis for optimism that the result predicts too high a carcinogenicity. Until such time that evidence dictates a change in estimate, any action based upon hopes the the risk estimates are

too conservative is clearly irresponsible public health practice. GESMO is necessarily a public health document in addition to its functions in describing other aspects of the use of mixed oxide fuel.

In the immediately preceding discussion , possible sources of correction of the Gofman estimates of the lung cancer hazard of inhaled plutonium by a factor of 2 or 3 one way or the other are considered. However any such possible corrections turn out will not alter the fact of the enormously greater carcinogenicity of plutonium as demonstrated by the Gofman analysis versus prior analyses of this problem. The plutonium carcinogenesis as estimated by Gofman is of the order of 1000 times greater than prior estimates, (or of some current "we hope and pray" estimates) So corrections of a factor of 2 or 3 will not alter the major revisions in all its thinking that NRC will have to do to bring GESMO anywhere near the realm of the real world .

APPENDIX

Responses by J.W. Gofman to the Minor Issues Raised in the ERDA Lab Critiques

1. Document: "Review of John W. Gofman's Reports on Health Hazards from Inhaled Plutonium" Chester R. Richmond, ORNL/TM- 5257, February 1976.

Point No. 1 Richmond states, p.2, " It would appear that Gofman is completely dismissing the hot particle arguments, yet it is not clear until one reads the paper that he leans heavily as he derives his risk estimates upon the argument of a large reduction in the mass of the presumed critical target tissues within the lung."

Answer Gofman had no need whatever to address the hot particle arguments in this work, so he had no occasion either to accept or dismiss hot particle arguments. A bit of reading would help Dr. Richmond on this mis-statement of his. Dr. Richmond also complains that one must read Gofman's papers in order for it to become clear what Gofman has done. I hardly think it is unusual to have to read a paper to understand it.

Dr. Richmond is indeed correct that Gofman leaned heavily upon the argument of a large reduction in mass of the presumed target tissues within the lung. In fact a reduction factor of 570, thoroughly justified by the fact that it is the bronchial cells one must consider if bronchogenic carcinoma is at issue! I doubt very seriously that Dr. Richmond can find any scientific support for inclusion of irradiation of blood vessels, nerves, alveolar tissue, terminal bronchiolar tissue , or other tissue in determination of the radiation effect upon bronchial cells in the production of bronchogenic cancer. I believe Dr. Richmond would make as much sense to include those extraneous tissues in the critical mass as to include the mass of the ass. There are some rare instances in carcinogenesis where indirect effects of radiation can have effects upon carcinogenesis. Thus radiation injury to a hormone-producing target may increase tropic hormone output of the pituitary and thus have influence. I doubt very much that we are dealing with anything like this in eliminating the extraneous lung tissues for the critical target assessment here. Second there is the relation of immune suppression that may operate to accelerate carcinogenesis. It is

inordinately doubtful that elimination of extraneous lung tissue from the mass of critical bronchial cells could possibly lead to overlooking a significant immune response in the carcinogenesis process in the bronchial cells.

Point No.2 Dr. Richmond states that if Gofman's $T_{1/2}$ of 500 days in the damaged bronchial areas were true,the"lungs of many heavy smokers would obviously become rapidly filling reservoirs for all sorts of atmospheric contaminants and particulates and perhaps, more important, if large regions (whatever anatomical reference this might have) were severely damaged by loss of cilia resulting in extremely long clearance half-times, the affected individuals would most probably drown in their own fluids." (Direct quote of Richmond)

Answer: Dr. Richmond has simply missed the entire point about what is being discussed. The subject, Dr. Richmond, is highly insoluble particulates of $PuO_2$and possibly other highly insoluble particulates. So Dr. Richmond's concern about fluids is simply absurd. Equally foolish is concern about "all sorts of atmospheric contaminants", a large proportion of which are soluble and hence not even at issue. Moreover, it is still a highly open question as to why PuO_2 particulates are retained so tenaciously when they are. The alpha activity has not been ruled out as a factor. The density of the particulates, very high for PuO_2 compared with most atmospheric insoluble particles is undoubtedly important. On a quantitative basis Dr. Richmond would be extremely hard-pressed to show that the insoluble, high-density, alpha-particle emitting particulates of atmospheric contaminants would, according to the Gofman thesis, lead to any blockage of the airway, to say nothing of such nonsense as "drowning in their fluids." Lastly I would advise Dr. Richmond to have a hard look at the Radford-Martell data discussed in the body of this testimony, which data suggest order-of-magnitude(at least) agreement with Gofman's $T_{1/2}$ values for the highly insoluble Lead-210-containing particulates from mainstream smoke of cigarettes.

Point 2a: Here I shall take up an expansion of Point 2 concerning possible airway blockage since in one form or another it occurs in all five of the ERDA Lab critiques. The Los Alamos critique comments on the amount of dust that might accumulate although there is not a claim of airway blockage. The other four critiques claim , outrightly

that airway blockage would occur if Gofman's $T_{\frac{1}{2}}$ of 500 days for highly insoluble PuO_2 particles were correct for injured regions of bronchial epithelium. This assertion is simply absurd, as will now be demonstrated in extenso.

In the Los Alamos critique, Healy et al state the following;

"Secondly, using his values for the quantity in the bronchial area (2.7%) retained with a long half-life (500 days) a mechanism of entry into the tissue would predict that, for a normal dust concentration of 100 $\mu g/m^3$, the one gram of bronchial epithelium would accumulate at equilibrium some 39 mg of dust."

Let us accept the correctness of the Healy et al calculation that the accumulation would be 39 milligrams. Healy et al then go on to state the following;

"Of course, in industrialized communities, the actual concentration in the air may be several times the 100 $\mu g/m^3$ assumed above."

Let us grossly exaggerate the problem by not only crediting the air concentration as "several times the 100 $\mu g/m^3$", but by assuming an air concentration of one hundred times the 100 $\mu g/m^3$, a rash assumption indeed. We would, therefore, have to multiply Healy's value of 39 mg of dust by 100, giving an accumulation of 3.9 grams of dust.

I'll accept, for rashness' sake, that Healy's calculation that all atmospheric dust will behave like insoluble PuO_2 aerosol is correct (rash, indeed, but let us assume it nevertheless). Our objective now is to estimate how much airway reduction occurs if the accumulation in the crucial bronchial areas is 2.7% as assumed by Gofman.

In CNR-1R, p.16, Gofman presented a value of 0.115 cm. as the radius of the relevant intrapulmonary bronchi. On p.17 of that same reference he calculated the surface area of such bronchi to total up to 361 cm^2.

The cross-sectional area of the lumen of such bronchi is πr^2, which for a radius of 0.115 cm is 3.1416 x $(0.115)^2$, or 0.0415 cm^2. Now let us calculate how much cross-sectional area is sacrificed to accomodate 3.9 grams of dust. Let us start by assuming a uniform distribution of the dust over the surface area of all the relevant bronchi, which is 361 cm^2. Let h = the height of the layer of dust. For mass equality, we have the following relation:

(Height) x (Area) x (Density) = Mass of dust.

As a reasonable value for the dust we shall use a density of 2 grams/ cm^3(noting that PuO_2 is much more dense than this). Substituting,

$$(h) \times 361 \times 2 = 3.9$$

$$h = 3.9/722 = 0.0054 \text{ cm.}$$

This value of h is, to a close approximation, equal to the radius reduction for these bronchi after dust accumulation. The new radius (post-dust collection) is 0.115-0.0054 = 0.1096 cm. The cross-sectional area of the reduced lumen is πr^2, or 3.1416 x $(0.1096)^2$, or 0.0377 cm^2. This means that the reduced lumen area is(0.0377/0.0415) times 100, or 90.8 % of the original lumen area. Expressed otherwise the loss in lumen area is only 9.2 %. This is so little reduction, even for the extremely unrealistic, rash assumptions used, that it is absurd to speak of airway blockage. The variations in lumen diameter from smooth muscle contractions in the bronchial wall will be far greater. Now the issue can be raised that the dust will only accumulate over the injured bronchial regions, so that the layer would be raised there. That is true, but irrelevant. Even if the "dust" is irregularly distributed, the approximate cross-sectional area lost is still the same.

The final conclusion that must be drawn is that the concerns of all the five ERDA Lab critiques concerning airway blockage is ridiculous indeed, since even making rashly worse assumptions than they did, the blockage is insignificant. Snipes et al, in the Lovelace Foundation critique ,made a calculation that if all the smoke inhaled with the smoking of 20 cigarettes per day were to behave like insoluble PuO_2 aerosols (an assumption that is not in the rash class, but rather in the wild blue yonder) then there would be about 6.4 grams accumulated in the relevant bronchi. This is followed by the completely guessstimated opinion that " Thus , ventilation would cease or be seriously impaired in heavy smokers." Had Snipes et al taken the trouble to calculate the airway reduction by the methods above , he would have found that the radius reduction is(6.4/3.9) x 0.0054, or 0.0089 cm, the reduced lumen area is 0.0354 cm^2, and the final area remaining is 85.2% of the original area (0.0354/0.0415) x 100), and this means a loss of 14.8 % of the airway cross-section----far, far from"ventilation would cease or be seriously impaired". And all this assumes some reasonableness for the assumption that all cigarette smoke behaves like insoluble PuO_2 aerosol.

Point 3: Richmond goes through a long discussion of Gofman's demonstration that Hempelmannoverestimated the body burden of the Manhattan Project workers by a factor of 14. I thought he was going to show that it was an incorrect criticism;instead he finally ended up admitting that many sources of new data would suggest that Hempelmann was wrong by a factor of ten in using urinary excretion data to estimate body burden. A factor of ten from experimental data is an excellent confirmation of Gofman's factor of 14. And as for the use Gofman made of the factor of 14, the same general conclusion would have been arrived at with a factor of 10. So this issue needs no further elaboration.

Point 4: In a wild, blatantly false statement on p. 12, Richmond states the following;

" The summary and conclusion section of Gofman's paper(CNR-1) states that there are 7.83 x 10^9 " lung cancer doses" per pound of plutonium . He neglects to point out in this section , however, that this estimate-- if true -- is per pound of plutonium deposited in the lung".

To show how ridiculously false Richmond's assertion is, I shall quote the summary and conclusions section on this subject from CNR-1 of Gofman:

" For cigarette-smokers:

Pu^{239}

(a) 0.058 micrograms deposited Pu^{239} represents one "lung cancer dose".
(b) 7,830,000,000 "lung cancer doses"per pound of Pu^{239}. "

It is hard to see how Gofman could have more explicitly indicated he was referring to DEPOSITED plutonium .He defined the " lung cancer dose" in terms of deposited plutonium , and that definition is used to give "lung cancer doses" per pound. What could be more straightforward? Perhaps an opthalmological examination is in order.

The follow-on to this nonsense is even worse in Richmond's critique. He says that the unsuspecting reader might incorrectly calculate 10^{13} to 10^{14} lung cancer deaths for the five to seven tons of plutonium produced as weapons fallout. On the very next page of Richmond's own report he then quotes Gofman's own calculation of 1,000,000 deaths. Any reader who calculates 10^{13} deaths, with a number of 10^6 deaths explicitly stated , would simply be totally incompetent.

Summary concerning Richmond's critique; There is absolutely nothing

that can be taken seriously in the Richmond critique. It should be totally and unequivocally rejected in its entirety.

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2. Document: "Comments Prepared by Dr. D. Grahn , Division of Biological and Medical Research, Argonne National Laboratory , Argonne, Illinois 60439, October 8, 1975.

Point 1: Grahn states the following on page 1 of his critique;

"Gofman then rederives risk in terms of " lung cancers per pound of plutonium " . This seems algebraically acceptable and technically it is a kind of extension of the "man-rem" concept. However, scientifically it is inappropriate because it does not allow for the all-important factors of atmospheric dispersion and dilution. "

Answer: This is sheer nonsense. Gofman derived all estimates of risk in terms of deposited plutonium, clearly , explicitly , and unequivocally. In no way can this be mis-interpreted as risk related to released plutonium. It should be totally obvious to Grahn that the factors of dispersion and dilution were clearly taken into account in all uses made by Gofman of the risk of lung cancer. All that can be said further to Dr. Grahn is,that when all else fails when one is criticizing a manuscript,it pays to read the manuscript.

Point 2: Grahn states the following on page 2 of his critique;

" A rather serious misconception is entrained in Gofman's discussion of the residence time of particulate matter on the bronchiolar epithelium in regions where cilia are absent. The assumption that epithelial residence half-time would be 500 days in the bronchioles , as employed for the alveolar regions , is not supported by evidence."

Answer: Gofman made abundantly clear that, for plutonium aerosols in the bronchi of cigarette smokers, there simply existed no evidence, and that, as a result, reasonable assumptions were being made. Indeed, since that time, the Radford-Martell evidence did appear concerning long residence half-times (see main body of this testimony) for insoluble particles in the bronchi of cigarette smokers. Of one thing we can be certain--- Dr. Grahn certainly presented no evidence to contradict the Gofman $T_{1/2}$ estimates. As he goes on in his discussion he cites the work of Albert et al in donkeys.

Several of the critiques cite the work of Albert, either for donkeys or humans, as evidence showing that a long clearance half-time cannot obtain for PuO_2 aerosols in the injured bronchial epithelium. The Los Alamos critique presents a detailed listing of the papers by Albert and his colleagues. It can be stated flatly in response to Grahn and all the others who cite Albert et al on this subject that the Albert work simply cannot address the question of a few percent long-term retention of insoluble aerosols in the bronchi. The very definition of bronchial clearance by Albert by his methodology would exclude long-term retention from the bronchi. It is to the credit of the Los Alamos group's critique that they recognized clearly that the Albert work cannot bear upon the Gofman thesis at all. They stated on page 7 of their critique the following:

" However, on careful examination these experiments would not detect the increased retention in the bronchial region postulated by Gofman since this fraction would be considered as pulmonary deposition and the normal fluctuation among individuals is too great to detect the Gofman assumption of 2.7% deposited in the bronchiolar region."

To this, I can only say "Amen, it is nice to see that somebody understands the literature." Richmond, Bair, Grahn, and Snipes, representing 4/5 of the ERDA Lab critiques , all grossly misuse the Albert data in their efforts to refute the Gofman thesis. It is regrettable that they indicated such a total lack of understanding of the problem at hand. Essentially the same can be said for the misuse by some of the critiques of related data of Lourenco et al.

Summary Concerning Dr. Grahn's Criticisms:

Aside from the items mentioned above,Dr. Grahn's comments and erroneous calculations are amply covered in the issues discussed in the main body of this testimony. I conclude that none of Dr. Grahn's points are valid in any way. I reject each and every one of his criticisms as erroneous.

Document 3: Bair, W.J. " Review of Reports by J.W. Gofman on Inhaled Plutonium" BNWL-2067/UC -41 , October 10,1975, Battelle Northwest Laboratories.

The major points of Bair's critique are thoroughly refuted in the main body of this testimony . Some of the minor points are those where Bair erred as did other ERDA laboratories,and they are covered with mention of Bair in the discussion of the specific points for the other ERDA critiques.

Point 1: Bair makes the point that it is possible that some of the plutonium alpha particle energy is lost in mucus or in metaplastic cells other than those which are subject to carcinogenesis.
Answer: As conceded in the main body of this testimony, Gofman agrees with Bair that it is entirely possible that some correction may have to be made for such effects. But neither Bair nor anyone else has presented any evidence that such a correction is of consequence or even an order of magnitude of possible size. Only some real data can help us here.

Point 2: Bair states(on page 9 of his critique)the following:

" If Gofman's premise that clearance of plutonium from lungs of smokers is greatly impaired is true , human autopsy data should be showing much higher lung burdens of plutonium in smokers than in non-smokers . This has not been revealed in the results published to date."

Answer: Patent nonsense! It would be a revelation indeed if Dr. Bair could point out a single published study of human autopsy material that has been studied in a manner to test whether insoluble PuO_2 aerosols are retained in cigarette smokers to the extent of 2.7% versus 0.2 % in non-smokers. This would have required meticulous analysis of relevant bronchi versus lung parenchyma. While such data would help resolve the problem, Dr. Bair seems unable to point such data out to the world. Moreover, the analysis of autopsy material obtained a long time after the exposure(more than a couple of clearance half-times) would be unsatisfactory even if a careful dissection of bronchial epithelium were made to compare with lung parenchyma.

Summary Concerning Dr. Bair's Criticisms:

The major points of Bair's criticisms are adequately dismissed as erroneous in the main body of this testimony since they deal with such issues as the $T_{\frac{1}{2}}$ of 500 days, the use of the linear hypothesis, and the BEIR Report estimate of the relative risk per rad --- all of which is disposed of there. The one point concerning a possible correction of alpha energy absorbed in mucus or non-relevant cells is a valid point and has been discussed above. There is nothing else in the Bair critique that is valid, and hence aside from this one point, all else in the Bair critique is rejected in toto.

Document 4: Snipes, M.B., Brooks, A.L., Cuddihy, R.G., and McClellan, R. O. "Review of John Gofman's Papers on Lung Cancer Hazard From Inhaled Plutonium" , L F-51/ UC-48 , Lovelace Foundation for Medical Education and Research, P.O.Box 5890, Albuquerque, NM 87115.

The major points of this critique have already been dismissed either in the main body of this testimony or in the discussion of other critiques which share the erroneous analysis of this critique. There is one point worth mentioning , ridiculous as it is. Incredibly, the Snipes critique states the following;

" Gofman states that his approach does not involve the "hot particle" hypothesis which precipitated considerable controversy in 1974. However, his approach to this problem is in fact an extension of the hot particle hypothesis."

Answer: "Incredible" is far too weak a word to describe this absurd assertion by the Lovelace workers. There are only two possibilities . First, the Lovelace workers don't have the foggiest notion of what the "hot particle" hypothesis is all about. Second, they don't understand anything they ostensibly read in the Gofman-Pu papers. On second thought, a third possibility must be entertained, namely, the Lovelace workers understand neither the "hot particle" hypothesis nor what they ostensibly read in the Gofman-Pu papers. (See footnote, p.26)

Summary Concerning the Lovelace Foundation Critique: It is impossible to find a single item in this critique that has any validity at all. Everything in this critique is therefore rejected unequivocally and totally.

Document 5 : Healy, J.W., Anderson, E.C. , McInroy, J.F., Thomas, R.G., and Thomas, R.L. " A Brief Review of the Plutonium Lung Cancer Estimates by John W. Gofman" , LA-UR-75-1779, Los Alamos Scientific Laboratory, Los Alamos, New Mexico , October 8, 1975.

There are a few minor points to address specifically in the Los Alamos critique. A general commentary follows in the Summary concerning this critique.

Point 1: The Los Alamos workers raise the question as to whether the Auerbach data can be directly described in terms of percent of cilia lost in cigarette smokers. The Auerbach data give a composite measure, namely percent of slides showing two features , loss of cilia plus an average of 4 or more cell rows in epithelial depth. According to this criterion of scoring slides, a slide with total absence of cilia but with fewer than four cell rows in depth would be scored as negative. Inasmuch as cilia loss is , in all likelihood , a lesser degree of damage than metaplasia or hyperplasia, cilia loss might be expected to be even more frequent a finding than is indicated in the Auerbach tables. Because of this uncertainty in the appropriate use of the Auerbach data, Gofman sought data that are unequivocal on the issue of per cent of cilia lost in smokers. Such data are published by Ide and co-workers, and are referenced in JAMA by Gofman. The Ide publication shows 30% ciliary loss in cigarette smokers, whereas Gofman used a value of 25 % as his interpretation of the Auerbach tables. Thus it turns out that any revision of the estimation of the lung cancer risk in smokers , using the Ide data,would make the plutonium risk somewhat worse than estimated by Gofman.

Point 2: The Los Alamos workers point out that some data indicate very long -term retention of part of the plutonium deposited in the lung--- much longer than would be suggested by a $T_{1/2}$ value of 500 days for clearance . They suggest, therefore, that Gofman's criticism of the Hempelmann estimates of body burdens in the Manhattan Project Workers may be somewhat too severe. Unfortunately , this finding of some very long-retained burden in the lung does not allow for such a simple interpretation. First of all , there are small lymph nodes in the lung, other than the large masses of nodes in the tracheo-bronchial region. It is entirely possible that some, or even most, of the residual burden is in lymphatic tissue and not in true lung

tissue. Second, even if the residual burden is in true lung tissue, there would be a world of difference if the burden is in the bronchiolo-alveolar region versus being in the bronchial tissue(that is , the bronchial tissue relevant for bronchogenic cancer.) If the burden is in the bronchiolo-alveolar tissue, the concentration of plutonium would be estimated to be 570 times lower there than in the bronchial tissue , with its corresponding marked reduction in radiation dose due to the factor of mass of tissue. Additionally , the intrinsic risk of bronchiolo-alveolar cancer, "spontaneously " is very much lower than that for bronchogenic cancer. Bair , in his critique, references DeLaRue et al,(Cancer, 29, 90-97-1972) as giving a value for bronchiolo-alveolar cancer representing only 3-6% of the total number of lung cancers both in smokers and non-smokers. Thus , if any especially long-term retention does exist , it is of the utmost importance to know precisely where in the lung it is. If it is in the parenchyma, the estimated risk by Gofman would hardly be much increased, contrasted with an estimated risk elevation that would be required if the long-term retained material is in the bronchi.

Summary Concerning the Los Alamos Lab Critique: There are a number of minor points that could be addressed additionally concerning the Los Alamos critique. However, the following quotation from the Los Alamos paper indicates it is not even worth going into these minor points in detail. The final section of the Los Alamos paper has the quotation on pages 17-18. I quote,

"We would recommend that measurements continue with more emphasis on the absolute bronchial retention, and that until such evidence is available, the Gofman predictions be regarded as interesting and imaginative speculations which should serve to stimulate increased interest in certain phases of current studies. "

Let us translate what the Los Alamos group is saying to ERDA. They are saying, in plain English, we don't have the ability to say whether Gofman's estimates are correct or not correct, and we aren't going to know until there are more data.

The message should be pretty clear for GESMO. The NRC draft of GESMO makes all kinds of comments about the radiological hazards of mixed oxide fuel use utilizing a hazard estimate for plutonium induction of lung cancer that may 1000-fold or so too low. In view of the Los Alamos admission on this point, there is no conclusion to

be drawn other than that which follows. The NRC draft of GESMO is simply incompetent on the issue of plutonium toxicity . If the NRC persists in using such an incompetent estimate of the lung cancer hazard of plutonium,plus fairy-tale dreams of what the release fraction of plutonium will be at reprocessing and fuel fabrication facilities, then the NRC will be declaring it has no respect whatever for the health and safety of the public.

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Footnote: In several of the ERDA Lab critiques ,reference has been made to the "hot particle" hypothesis of Geesaman-Tamplin-Cochran. Indeed some of the ERDA Lab critiques suggest that Gofman's thesis makes use of, or is a variant of,the "hot particle" thesis. Such an assertion is blatant nonsense of the very worst kind. Those who make such an assertion show a total lack of understanding of the "hot particle" thesis and the Gofman hypothesis.

The ERDA Lab critiques which infer that Gofman made use of the "hot particle" hypothesis do so because Gofman calculated the mass of the relevant bronchial tissue (for carcinogenesis)to be one gram rather than the 570 grams of the whole(bloodless) lung. In using one gram Gofman is stating a most simple scientific fact, namely, if the bronchial cells represent the target for carcinogenesis, one calculates the dose to that target , not to miscellaneous and sundry assorted garbage. It is pathetic to find this totally straightforward scientific operation misconstrued as use of the "hot particle" thesis to which it bears exactly zero relationship.

The "hot particle" thesis predicts that as the dose to a so-called "critical architectural unit" increases, the effectiveness of radiation increases far more than one would anticipate from linearity because a new mechanism of carcinogenesis has entered the picture. In striking contrast,the Gofman approach says the situation is precisely what linear theory would predict, namely if the mass of target is reduced for a given radiation source, the increase in carcinogenic effect of radiation is in the same proportion as is the decrease in mass of the target tissue. This is precise use of the linear hypothesis, and is in no way related to the "hot particle" hypothesis.

In the Los Alamos critique it is stated that,since only 25% of the one gram of bronchial epithelium is injured, and hence the plutonium concentrates its radiation there, this constitutes use of the "hot particle" thesis. Sheer, unadulterated nonsense! If the plutonium concentrates in ¼ of the bronchial epithelium and delivers its dose there, the carcinogenic effect is 4 times as high there as it would be for the plutonium distributed over the whole gram of bronchial tissue. Thus, in terms of effect, $\frac{1}{4} \times 4 = 1$, and $1 \times 1 = 1$. This shows that the plutonium concentrating in ¼ of the cells gives exactly as much carcinogenic effect as that same amount of plutonium distributed in all the cells. This is exactly what linear theory predicts and it is linear theory that Gofman uses. None of these considerations even remotely resemble any aspect of the "hot particle" thesis.

Curriculum Vitae (March, 1977)

John W. Gofman, M.D., Ph.D.

Vital Statistics

Birth: September 21, 1918 in Cleveland, Ohio.

Education:

Grade and High School in Cleveland, Ohio

A.B. Oberlin College, Chemistry , 1939

Ph.D. in Nuclear Chemistry, University of California at Berkeley, 1943.

Dissertation: The Discovery of Pa^{232}, U^{232}, Pa^{233}, and U^{233}
The Slow and Fast Neutron Fissionability of U^{233}
The Discovery of the 4n+1 Radioactive Series.

M.D. School of Medicine, University of California at San Francisco, California, 1946 (First year of medicine at Western Reserve University , Cleveland, Ohio)

Internship in Internal Medicine, University of California Hospital, San Francisco, California, 1946-1947.

Positions: Academic appointment in the Division of Medical Physics, Department of Physics, University of California at Berkeley, California in 1947.

Advancement to the Full Professorship in 1954, holding that position to present, with shift to Emeritus status in December, 1973.

Concurrent appointment during that entire period as either Instructor or Lecturer in Medicine in the Department of Medicine, University of California, San Francisco.

Additional Appointments held:

(1) Associate Director, Lawrence Livermore Laboratory, 1963-1969. Resigned this post to return to full-time teaching and research. Remained as Research Associate at Lawrence Laboratory through February, 1973.

(2)Founder and First Director of the Biomedical Research Division of the Lawrence Livermore Laboratory, 1963-1965. This work was done at the request of the Atomic Energy Commission for the purpose of establishing a program of overall evaluation of the effects of all types of nuclear energy activities upon man and the biosphere.

(3) Medical Director , Lawrence Laboratory, 1954-1957.

(4) Medical Consultant to the Aerojet-General Nucleonics Corporation, with special emphasis on the hazards of ionizing radiation, for approximately 8 years during the 1960's.

(5) Member of the Nerva Advisory Board (Nuclear Engine Rocket Vehicle Application) supervising the activities of the Westinghouse and Aerojet-General Corporations in the Federal program for nuclear propulsion in space, approximately 3 years during the 1960's.

(6) Consultant to the Research Division of The Lederle Laboratories, American Cyanamid Corporation, 1952-1955.

(7) Consultant to the Research Division of The Riker Laboratories for approximately 4 years, 1962-1966.

Curriculum Vitae--John W. Gofman

Honors and Awards:

1. Goldheaded Cane Award, 1946, Presented to Graduating Senior in Medicine, University of California Medical School, for qualities as a physician.
2. Modern Medicine Award, 1954, for outstanding contributions to heart disease research.
3. The Lyman Duff Lectureship Award of the American Heart Association in 1965 for research in atherosclerosis and coronary heart disease.
4. The Stouffer Prize, 1972, shared $50000 prize and Gold Medal for outstanding contributions to research in arteriosclerosis.
5. American College of Cardiology, 1974, Selection as one of 25 leading cardiology researchers of the past quarter-century.

Publications:
Approximately 150 scientific publications in leading scientific journals encompassing the following fields;

1. Lipoproteins, atherosclerosis, and coronary heart disease.
2. Ultracentrifugal discovery and analysis of the serum lipoproteins.
3. Characterization of familial lipoprotein disorders.
4. The Determination of Trace Elements by X-ray Spectrochemical Analysis.
5. The Relationship of Human Chromosomes to Cancer.
6. The Biological and Medical Effects of Ionizing Radiation, with particular reference to cancer, leukemia and genetic diseases.
7. The Lung Cancer Hazard of Plutonium.
8. Problems Associated with Nuclear Power Production.

Patents:

1. The slow and fast neutron fissionability of Uranium^{233}, with its application to production of nuclear power or nuclear weapons.
2. The Sodium Uranyl Acetate Process for the Separation of Plutonium from Uranium and Fission Products from Irradiated Fuel.
3. The Columbium Oxide Process for the Separation of Plutonium from Uranium and Fission Products from Irradiated Fuel.

(These latter two patents eventuated from research conducted during tenure as Group Leader, The Plutonium Project at the University of California, Manhattan Project, 1942-1944)

Teaching:

1. The application of radioactive tracers to chemical, biological, and medical problems.
2. The biological and medical effects of ionizing radiation.
3. Mechanisms of cancer induction.
4. Atherosclerosis and heart disease.
5. Environmental factors in the induction of cancer.
6. Research Guidance of some 25 students toward the doctorate in biophysics or medical physics.

Books:

1. What We Do Know About Heart Attacks
2. Dietary Prevention and Treatment of Heart Disease (with Alex V. Nichols and Virginia Dobbin)
3. Coronary Heart Disease
4. Population Control Through Nuclear Pollution(with Arthur Tamplin)

Books (Continued)

5. Poisoned Power: The Case against Nuclear Power, (with Arthur Tamplin).
6. Contributor of Chapters to numerous Books, including some on nuclear engineering, cancer induction, biochemical and biophysical methods, heart disease, and effects of radiation.
7. Co-Editor of the Advances in Biological and Medical Physics, with John H. Lawrence and Thomas Hayes, a multi-volume series published by Academic Press.

Invited Testimony

1. Presentations invited by the Joint Committee on Atomic Energy of the United States Congress , all of which was published in the form of 13 papers in the Hearings of the Joint Committee on Atomic Energy , 91st Congress of the United States, 1970.

2. Presentation before the Select Committee on Nuclear Energy Generation , The Pennsylvania State Senate, Sept. 2, 1970.

3. Presentation before the Committee Investigating the Barnwell Fuel Reprocessing Center, South Carolina Legislature, Columbia, South Carolina, January, 1972.

Current Work

1. Continuation of Research on the Biological Hazards of Plutonium (as part of my research as Emeritus Professor, University of California)
2. Guidance of Ph.D. Research Dissertations of Students in the Biophysics Program of the University of California (as part of my work as Emeritus Professor).
3. Independent Consulting
4. Continuation of Research on the Cancer and Leukemia Induction by Ionizing Radiations from radionuclides and x-ray sources (part of my research as Emeritus Professor).
5. Chairman, The Committee for Nuclear Responsibility(Uncompensated Public Interest Work).
6. Publication of Scientific Papers on Current Research.

FRIENDS OF THE EARTH
124 SPEAR SAN FRANCISCO CALIFORNIA 94105
415 495-4770

April 10, 1979

Mr. John Farmakides
U.S. Department of Energy
Hearing Board on Livermore DEIS
Oakland, California

Dear Sir;

Enclosed are portions of the testimony which Friends of the Earth will be presenting at the DOE hearing in Livermore on Thursday, April 12th, 1979, regarding the seismic hazards to the DOE labs there. Other testimony is in preparation concerning the structural designs and seismic criteria and the radiological dose assessments and health physics. These will be presented in detail during the hearings.

Friends of the Earth hereby requests the Department of Energy to remove all of the plutonium and other radioactive materials from DOE's nuclear weapons labs in Livermore because of the extreme hazard to public health and safety created by the existence of 13 active earthquake faults which could damage the critical structures at the labs.

An earthquake on one of these 13 active faults could cause severe structural damages to the nuclear labs and to the nuclear reactor in Livermore. The earthquake damages to the labs could release huge amounts of radioactive cesium, curium, plutonium, tritium, iodine gases, and other radioactive materials into the Bay Area environment. The radiation could contaminate farmlands, residential communities, and the nearby reservoirs and aqueducts that supply drinking water to San Francisco, Peninsula cities, and San Jose. The South Bay Aqueduct is an open canal across the street from the labs. Apparently, several hundred pounds of plutonium are handled at the labs each year. Huge amounts of radioactive wastes are produced and stored temporarily at the labs. Some plutonium was already leaked from evaporator tanks.

If an earthquake damaged facilities at Livermore, clouds of radioactive gases could be released. Such a nuclear emergency could require the evacuation of many of the four and a half million people who live within a forty mile radius of the Livermore labs. This radioactive disaster could be much worse than the recent accident in Pennsylvania because evacuations of Bay Area populations would be complicated by damages to freeways and other transportation routes during an earthquake. Even if the freeways did remain intact, the evacuation plans are inadequate and unrehearsed. The counties and cities that would be involved are not prepared for evacuations.

The DOE staff has suggested that Livermore Lab structures with the potential for adverse environmental impact could be relocated away from the San Francisco Bay Area. This alternative of partial relocation to mitigate environmental hazards was suggested on page 5-2 of the DEIS. The staff there suggests that the Nevada Test Site would be an attractive location for this partial relocation because it is remote, owned by DOE, and already contaminated by weapons tests.

Friends of the Earth believes that this suggestion of partial relocation is the best solution to the current situation in which millions of people are endangered by the earthquake hazards to the labs. Thus, Friends of the Earth strongly urges the DOE to adopt this alternative for mitigation of hazards. All plutonium and other radioactive materials should be removed from the active fault zones in the Livermore Valley.

EARTHQUAKE HAZARDS TO THE LIVERMORE LABS

Two issues of immediate concern to both the DOE and the Friends of the Earth are the potential ground motions beneath the critical structures at the Livermore Labs and the possibility of surface rupture beneath these buildings during an earthquake. Ground motions during earthquakes are measured in terms of "g" values.

Two seismic studies done by Livermore Lab and Berkeley engineering geologists (LLL reports UCRL-51193 and UCRL-51592) concluded that the labs could experience a 0.8 g during an earthquake on nearby faults. However, the DEIS (page 2A-29) reveals that the LLL management chose to adopt a less conservative estimate of 0.5 g that was suggested by a third consultant who was hired to produce a report that would minimize the amounts of structural modifications that would be necessary to satisfy concerns.

Recently at the nearby Vallecitos Nuclear Center, the NRC adopted a potential ground motion estimate in excess of 1.0 g for a site 7 miles west of Livermore.

Friends of the Earth presents enclosed testimony for your consideration from Dr. Jim Brune, which says that "Rupture along the Tesla fault, as well as along other mapped faults in the region, in the direction of the Livermore site, could result in anomonously high acelerations in excess of 2.0 g."

Friends of the Earth also submits an attached article written by Dr. M. D. Trifunac, Professor of Engineering at the University of Southern California, and published in the Bulletin of the Seismological Society of America, that estimates, with 90% confidence, that it would be possible to have ground motions of up to 3.0 g in soil conditions similar to those at Livermore, if subjected to a 6.5 magnitude quake at 0 distance. This can be calculated from Table 7 in the enclosed report.

In several reports it has been estimated that the Tesla fault, the Las Positas fault and other faults near the labs are capable of generating 6.5 magnitude earthquakes. If such an event occurred, then the high ground accelerations could result and cause damages to the critical structures at the labs. The result could be serious contaminations and health hazards.

It will be interesting to see how the DOE responds to this urgent situations involving the public health and safety of so many people in this region. In the light of the recent events in Pennsylvania, will the DOE continue to act in the arrogant manner of its predecessor, the AEC, with total disregard for public health and safety? Will the DOE defend the Livermore site regardless of the seismic hazards, or will the DOE consider the alternative of partial relocation of dangerous radioactive materials to the NTS away from the homes and drinking water supplies of millions of Bay Area residents?

UNIVERSITY OF CALIFORNIA, SAN DIEGO

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INSTITUTE OF GEOPHYSICS AND
PLANETARY PHYSICS, A 025
SCRIPPS INSTITUTION OF OCEANOGRAPHY

LA JOLLA, CALIFORNIA 92093

April 6, 1979

Mr. John Farmakides
U.S. Department of Energy
Washington, D.C. 20545

Dear Sir:

I am responding to a request by Mr. Andrew Baldwin, representing Friends of the Earth, that I comment on the seismic design appropriate for the Lawrence Livermore site. My comments are based on the geologic situation as presented in the LLL report by L. H. Wight, "A Geological and Seismological Investigation of the Lawrence Livermore Laboratory Site", and on present knowledge concerning peak ground accelerations expected very close to earthquakes, as presented in my recent testimony before the NRC concerning the Diablo Canyon Nuclear Power Plant. This testimony is attached.

The essential conclusion of my testimony before the NRC was that for large earthquakes (M > 7) we do not have a sufficient data base nor physical understanding to predict ground accelerations very near fault breaks (< 10 km distance) with confidence. Available data and physical understanding indicate that accelerations of greater than 2 g are possible and accelerations of greater than 1 g may be common.

One aspect of the problem discussed in some detail in my testimony, and which may be of crucial importance to the Livermore site, is the phenomenon of directivity focussing of energy in the direction of fault propagation. Rupture along the Tesla fault, as well as along other mapped faults in the region (in the direction of the Livermore site), could result in anomalously high accelerations (> 2g). It is not possible to accurately assess the probability of such an anomalously high acceleration, but the effect is well established and commonly observed in rupture propagation (see pp 3-10 to 3-14 of my testimony).

Also of particular importance to the Livermore site is the conclusion of Ambreysey's that accelerations of greater than 1 g will probably be recorded for even low magnitudes (p 3-8). On April 6, 1977 a magnitude 5.5 shallow earthquake in Iran generated peak accelerations of .95 g and 1.08 g, horizontal and vertical components respectively. (S - T $\sim$ 1 sec.)

Another part of my testimony which is of critical importance to the Livermore site is the reported results from the Victoria Baja California earthquake swarm of March 1978 (Appendix II of my testimony, pp II-1 to II-7). One event of magnitude 4.9 produced accelerations of about .64 g at a distance of about 10 km. Although final information on the depth, location and mechanism of the event are not yet available, it nevertheless shows that even relatively small events can generate accelerations of over .6 g in an environment of very thick alluvium. This result indicates that the acceleration value of .5 g taken in the L. H. Wight report is not conservative.

Sincerely,

James N. Brune
Professor of Geophysics

JNB:sd

BIOGRAPHICAL SKETCH

(PROVIDE THE FOLLOWING INFORMATION FOR ALL PROFESSIONAL PERSONNEL ENGAGED ON THE PROJECT, BEGINNING WITH THE PRINCIPAL INVESTIGATOR)

NAME James N. Brune

BIRTHDATE (MO., DAY, YR.)

November 23, 1934

PLACE OF BIRTH (CITY, STATE, COUNTRY)

Modesto, California U.S.A.

PRESENT NATIONALITY (ALIENS INDICATE KIND OF VISA AND EXPIRATION DATE)

U.S. Citizen

EDUCATION (BEGIN WITH BACCALAUREATE TRAINING AND INCLUDE POSTDOCTORAL)

DEGREE	YEAR CONFERRED	INSTITUTION AND LOCATION
B.Sc.	1956	University of Nevada, Reno, Nevada
Ph.D.	1961	Columbia University, New York City

HONORS AND AWARDS

See Attached

MAJOR RESEARCH INTEREST

Earthquake Source Mechanism
Tectonics
Earth Structure

RESEARCH AND/OR PROFESSIONAL EXPERIENCE (STARTING WITH PRESENT POSITION, LIST PROFESSIONAL BACKGROUND AND EMPLOYMENT)

Professor of Geophysics-University of California, San Diego, 1969 -
Associate Director, Institute of Geophysics and Planetary Physics, University of California, San Diego, 1973 - 1976.
Chairman, Geological Research Division, Scripps Institution of Oceanography, University of California, San Diego, 1974 - 1976.
Associate Professor of Geophysics-California Institute of Technology, 1965 - 1969.
Adjunct Associate Professor of Geology-Columbia University, 1964.
Geophysicist, U. S. Coast and Geodetic Survey, 1964.
Research Scientist, Columbia University, 1958 - 1963.
Exploration Research, Chevron Oil Company, 1957.
Exploration Geophysics, Chevron Oil Company, 1956.

James N. Brune

HONORS

Higgins Fellowship, Columbia University, 1956
University Fellowship in Geophysics, Columbia University, 1957-58
Max Fleischman Scholarship, University of Nevada, 1954-55
Jones-Hoover Scholarship, University of Nevada, one year
J. B. MacIlwane Award of American Geophysical Union, 1962
Fellow of the American Geophysical Union, 1967
Grove Karl Gilbert Award in Seismic Geology, 1967
Seismological Society of America: Board of Directors, 1967-present
Vice-President, 1969, President, 1971
Member of New York Academy of Sciences, 1970
Arthur L. Day Award, 1972
G. K. Gilbert Award, Carnegie Institution of Washington, 1967
Listings in *Who's Who in the West, American Men of Science*
Fellow Geological Society of America
Member, California Earthquake Prediction Evaluation Council

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James N. Brune

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PATRICK M. GRIFFIN
1555 Sonoma Avenue
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April 6, 1979

Mr. John Farmakides
U.S. Department of Energy
Washington, D.C. 20545

Subject: Public Hearing at Lawrence Livermore Laboratory,
Livermore, California on April 12, 1979

Dear Mr. Farmakides,

I am a geotechnical engineer with BS and MS degrees from the University of California at Berkeley. I have worked since 1970 in the area of earthquake engineering, primarily in earthfill dam safety analysis. I am presently completing my PhD thesis in earthquake engineering at Berkeley.

I have just had the opportunity to review "A Geological and Seismological Investigation of the Lawrence Livermore Laboratory Site," by L. H. Wight, June 3, 1974, and wish to record several technical comments on that report.

In the Seismologic Evaluation section some care is taken to predict the Safe Shutdown Earthquake (SSE) for the site. For convenience, the author separates the site response to earthquakes into two catagories: large distant earthquakes and surface rupture on nearby faults. In calculating an approximation of the magnitude and accelerations from nearby earthquakes, a procedure is used involving the estimation of fault rupture length of nearby faults, and correlating these with recorded rupture length - earthquake magnitude and acceleration data. This procedure is often used to obtain a rough approximation of earthquake characteristics.

In this particular case, fault rupture lengths are particularly difficult to approximate because the actual fault rupture is beneath deep alluvial deposits. Surface expressions of faulting may have little relation to true bedrock fault activity.

Even more serious, however, is the fact that the various branch faults near the LLL site are all part of the extremely active San Andreas Fault system. Irrespective of recorded earthquake activity in the immediate LLL area, any of these faults could experience a major earthquake. This possibility appears to have escaped the attention of the author.

When discussing the response of the site to large distant earthquakes, the author chooses a scaled version of the Taft record of the Kern County Earthquake of 1952. There are several concerns that come to mind with the particular scaled record used. The author made no effort to relate the subsurface strata between Kern County and the Taft site to that at the San Andreas to LLL site. Different bedrock characteristics can significantly influence the frequency and attenuation characteristics along the path of the elastic waves. Furthermore, the selected duration of strong ground motion, less than 10 seconds, seems unconservatively short considering that the San Francisco Earthquake of 1906 had a duration of approximately 60 seconds.

The author proceeds to evaluate the ground surface response by using a lumped-mass analysis using equivalent linear soil properties. This particular type of analysis approximates the ground surface response through soil layers of vertically propagating shear waves. Among other things, the analysis assumes that the soil is horizontally bedded in continuous layers, and that the subsurface bedrock is horizontal and continuous over a large distance. This is clearly not the case at the LLL site. On page 2A-14 of the report, the author describes the site geology as follows:

> "The site is at the eastern edge of the Livermore Valley near the western limb of the Altamont anticline. As Fig. 8 illustrates, the geology in this area is particularly diverse. The oldest rocks in the area, the Franciscan, outcrop in the Diablo Range south of the site. These are overlain by successively younger strata toward the site. The Altamont anticline to the east is largely Cretaceous and Tertiary, while the site is on Quaternary alluvium."

Estimates of the depth of bedrock across the LLL site range between 400 feet and 1000 feet, indicating that the site is very probably underlain by a steeply sloping anticline. The lumped-mass method of analysis selected by the author would appear to be inappropriate for even an approximate response analysis for this site. Given the site geology, it is altogether possible that a finite element analysis, including accurate representation of bedrock and soil strata, would disclose locations on the site where significant amplification of accelerations will be predicted. Clearly, to adequately predict the ground surface response to any bedrock shaking, it will be necessary to conduct a more detailed subsurface investigation and conduct a more contemporary analysis of the site than the lumped-mass method.

The author's statement on page 2A-25 of the report that the method is appropriate for this site because "... the topography is nearly flat ..." appears to miss the very important limitations of the lumped-mass method of analysis.

On the same page the author states that liquefaction "presents no hazard ..." on this site, citing an earlier study by R. C. Murray and F. J. Tokarz, 1973. Since this report is unavailable to me I cannot comment on it; however, I would like to note that the fine-grained alluvial sands which appear to be characteristic of the site can be highly susceptible to liquefaction if they are loosely deposited and if they are in a high water table environment. This is particularly true if the site is subjected to long duration (more than 30 seconds) seismic shaking. I therefore suggest that the possibility of soil liquefaction on this site not be dismissed lightly.

The last comment I wish to make concerns the author's treatment of the site response during an earthquake on a nearby fault. As he states on page 2A-26, "the distinguishing characteristic of earthquakes on the nearby faults is the effect of surface rupture." What he does not state is that since the site is underlain by deep alluvium, the surface expression of faulting (i.e. -the actual ground surface rupture) may appear anywhere on the site, not necessarily directly above the bedrock faulting. Although there is very little data concerning accelerations in the immediate vicinity of a ground surface rupture, the author noted that "there are indications ... that the accelerations near surface ruptures are significantly higher than attenuation curves would indicate." (page 2A-26). It would appear that any critical structures on the site should be designed to withstand not only the SSE shaking, but also significant surface rupture and associated higher accelerations.

Thank you for this opportunity to comment.

Sincerely yours,

Patrick M. Griffin

Patrick M. Griffin

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PRELIMINARY ANALYSIS OF THE PEAKS OF STRONG EARTHQUAKE GROUND MOTION—DEPENDENCE OF PEAKS ON EARTHQUAKE MAGNITUDE, EPICENTRAL DISTANCE, AND RECORDING SITE CONDITIONS

By M. D. Trifunac

ABSTRACT

Analyses of peak amplitudes of strong earthquake ground motion have been carried out with the emphasis on their dependence on earthquake magnitude, epicentral distance, and geological conditions at the recording site. Approximate empirical scaling functions have been developed which, for a selected confidence level, yield an estimate of an upper bound of peak accelerations, velocities, and displacements. The parameters in these scaling functions have been computed by least-squares fitting of the recorded data on peak amplitudes which are now available for a range of epicentral distances between about 20 and 200 km and are representative for the period from 1933 to 1971 in the Western United States.

The possibility of extrapolating the derived scaling laws to small epicentral distances where no strong-motion data are currently available has been tested by comparing predicted peak amplitudes with related parameters at the earthquake source. These source parameters (average dislocation and stress drop) can be derived from other independent studies and do not contradict the inferences presented in this paper. It has been found that for an approximate 90 per cent confidence level the presently available data suggest that peak accelerations, velocities, and displacements at the fault and for the frequency band between 0.07 and 25 Hz probably do not exceed about 3 to 5 *g*, 400 to 700 cm/sec, and 200 to 400 cm, respectively.

The logarithms of the peaks of strong ground motion seem to depend in a linear manner on earthquake magnitude only for small shocks. For large magnitudes this dependence disappears gradually and maximum amplitudes may be achieved for $M \approx 7.5$. The influence of geological conditions at the recording site appears to be insignificant for peak accelerations but becomes progressively more important for peaks of strong-motion velocity and displacement.

Introduction

Since the early 1940's, numerous empirical scaling functions have been proposed and developed for prediction of peak ground acceleration as a function of earthquake magnitude and epicentral distance. Some other pertinent parameters which can be related to the characteristics of the recording sites have also been considered by several authors (Gutenberg and Richter, 1942; Newmann, 1954; Gutenberg and Richter, 1956; Blume, 1965; Housner, 1965; Kanai, 1966; Milne and Davenport, 1969; Esteva, 1970; Cloud and Perez, 1971; Donovan, 1972; Page *et al.*, 1972; Schnabel and Seed, 1973; Boore, 1973; Dietrich, 1973; Katayama, 1974). In our recent paper (Trifunac and Brady, 1975b) most of these scaling functions were reviewed and compared with the trends indicated by the strong-motion data now available for the Western United States. In the same paper it was shown that (a) peaks of strong ground motion do not grow linearly with magnitude, which

is in agreement with some previous investigations, and (b) that the soil conditions at a recording station have significant effect only for peak ground displacement and only minor to insignificant effect on peak accelerations and peak velocities. Finally, the new scaling functions which give the expected values and standard deviations for peaks of strong ground motion were presented.

The purpose of this paper is to extend this work by attaching approximate confidence levels to predicted peak amplitudes and to examine whether it is possible to develop a rational physical basis for the extrapolations of currently available results back to the earthquake source. It is felt that the computation of such approximate confidence levels for predicted peaks of strong ground motion may be useful in earthquake engineering applications, since it leads to more accurate and better defined procedures for scaling strong-motion amplitudes when the acceptable risk of exceeding a given level is known or is specified. Furthermore, the characterization of strong earthquake ground motion by the mean and standard deviation only of its peak amplitudes may not be adequate for some applications, since the distribution of measured amplitudes is not accurately known.

It has been found (Trifunac and Brady, 1975b) that the empirical law of the attenuation with distance (Richter, 1958), which is used for calculations of local earthquake magnitude, describes approximately the attenuation with distance of peak acceleration, velocity, and displacement data, but the adequacy of this approach for epicentral distances less than about 20 km has not been established. One of the main objectives in this paper is, therefore, to examine this problem in some detail. This can be done by comparing the predicted average dislocations and stress drops from the analysis of strong-motion data, which is carried out in this paper, with the independent estimates of these quantities derived from three detailed source mechanism studies which dealt with the corresponding geographical area (Trifunac, 1972a, 1972b; Thatcher and Hanks, 1973).

Finally, it should be noted here that the aim of this paper is to present the author's most recent assessment of what the actual recorded amplitudes of strong earthquake ground motion might be in the near-field. Although an effort is made in this work to test these estimates of predicted peak levels by comparing the results with independent calculations which are based on an approximate source mechanism theory, it must be remembered that the final test of the preliminary results we present in this paper can only come from future records of strong ground motion. While our present calculations may prove useful in earthquake engineering applications which require an estimate of actual strong-motion amplitudes, it should be emphasized that the strong-motion amplitudes discussed in this paper do not represent the design amplitudes for direct use in routine earthquake engineering calculations.

Strong-Motion Data

The amplitudes of peak acceleration, velocity, and displacement which are used in this study have been extracted from the Volume II tapes (Trifunac and Lee, 1973) which contain corrected accelerograms (Trifunac, 1971, 1972c) and integrated velocity and displacement curves (Hudson *et al.*, 1971). These data result from a strong-motion recording program in the Western United States and have been processed for the period beginning in 1933 and ending in 1971. The data consist of 187 accelerograph records (373 horizontal and 187 vertical components) which were obtained at "free-field" stations or in the basement floors of buildings. These data result from 57 earthquakes whose magnitudes range from 3.0 to 7.7. Of 187 records 6 or 3 per cent correspond to the magnitude range 4.0–4.9, 41 or 22 per cent to 5.0–5.9, 130 or 71 per cent to 6.0–6.9 and 7 or 3 per cent to the magnitude range 7.0–7.9. Sixty-three per cent of the data have been

recorded on alluvium sites (identified by $s = 0$ in this paper), 23 per cent on "intermediate" sites (identified by $s = 1$), and only 8 per cent on basement rock sites (identified by $s = 2$). Geological descriptions of these sites and the method employed to arrive at the final site classification used in this and our previous papers have been presented by Trifunac and Brady (1975a) and will not be repeated here.

Figures 1, 2, and 3 present the vertical and horizontal peaks of strong-motion data plotted versus epicentral distance in kilometers. Each peak is identified by its site classification symbol, "0", "1", or "2", and by the corresponding earthquake magnitude. Continuous curves in these figures represent predicted peak amplitudes for selected earthquake magnitudes equal to 4.5, 5.5, and 6.5, for the three site classifications and for a 90 per cent confidence level. The method for calculating these amplitudes will be discussed later in this paper.

Proposed Scaling for Peaks of Strong Ground Motion

It was recently suggested that the peaks of strong ground motion might be scaled by using the following expression

$$\log_{10}\begin{Bmatrix}a_{\max}\\ v_{\max}\\ d_{\max}\end{Bmatrix} = M+\log_{10} A_0(R)-\log_{10}\begin{Bmatrix}a_0(M)\\ v_0(M)\\ d_0(M)\end{Bmatrix} \tag{1}$$

(Trifunac and Brady, 1975b). In (1) $a_{\max}$, $v_{\max}$, and $d_{\max}$ represent peak acceleration, peak velocity, and peak displacement, respectively; M is earthquake magnitude, which in most cases is represented by the local magnitude M_L (Richter, 1958); $\log_{10} A_0(R)$ is the empirically determined function which describes attenuation versus distance (Table 1); and $a_0(M)$, $v_0(M)$, and $d_0(M)$ represent the magnitude-dependent empirical scaling functions for acceleration, velocity, and displacement.

Equation (1) is based on the assumption that $\log_{10} A_0(R)$ (Richter, 1958) describes approximately the amplitude attenuation with distance, R, for all peaks; i.e., peaks of acceleration, velocity, and displacement. It may seem at first that it is essential to have different attentuation curves for acceleration (high-frequency waves), velocity (intermediate-frequency waves), and displacement (low-frequency waves) peaks, but we found that for the limited number of data points that are available so far $\log_{10} A_0(R)$ may represent a satisfactory first-order approximation for all peaks of strong ground motion (Trifunac and Brady, 1975b) for the distance range from about 20 to 200 km.

For the scaling of peak amplitudes presented in this paper, we will neglect the dependence of $\log_{10} A_0(R)$ on wave frequency, recording site conditions, peak amplitudes, earthquake magnitude, and or on source dimensions and will use the values of $\log_{10} A_0(R)$ which are employed in routine calculations of local magnitudes (Richter, 1958). For large earthquakes which are characterized by long faults and large peak amplitudes, the $\log_{10} A_0(R)$ curve would have a tendency to flatten out for small epicentral distances; while for low magnitude shocks which are typically not accompanied by large source dimensions and large peak amplitudes, this curve would probably have a larger negative slope for small epicentral distances than the curve we propose to use (Table 1). Such general trends would result from the properties of geometric attenuation with distance and possibly nonlinear response of soil deposits for large near-field motions. The detailed quantitative description of these effects, however, is beyond the scope of this paper, since the strong-motion data for epicentral distance less than about 20 km, which would be necessary to test the adopted attenuation laws, is completely lacking at this time.

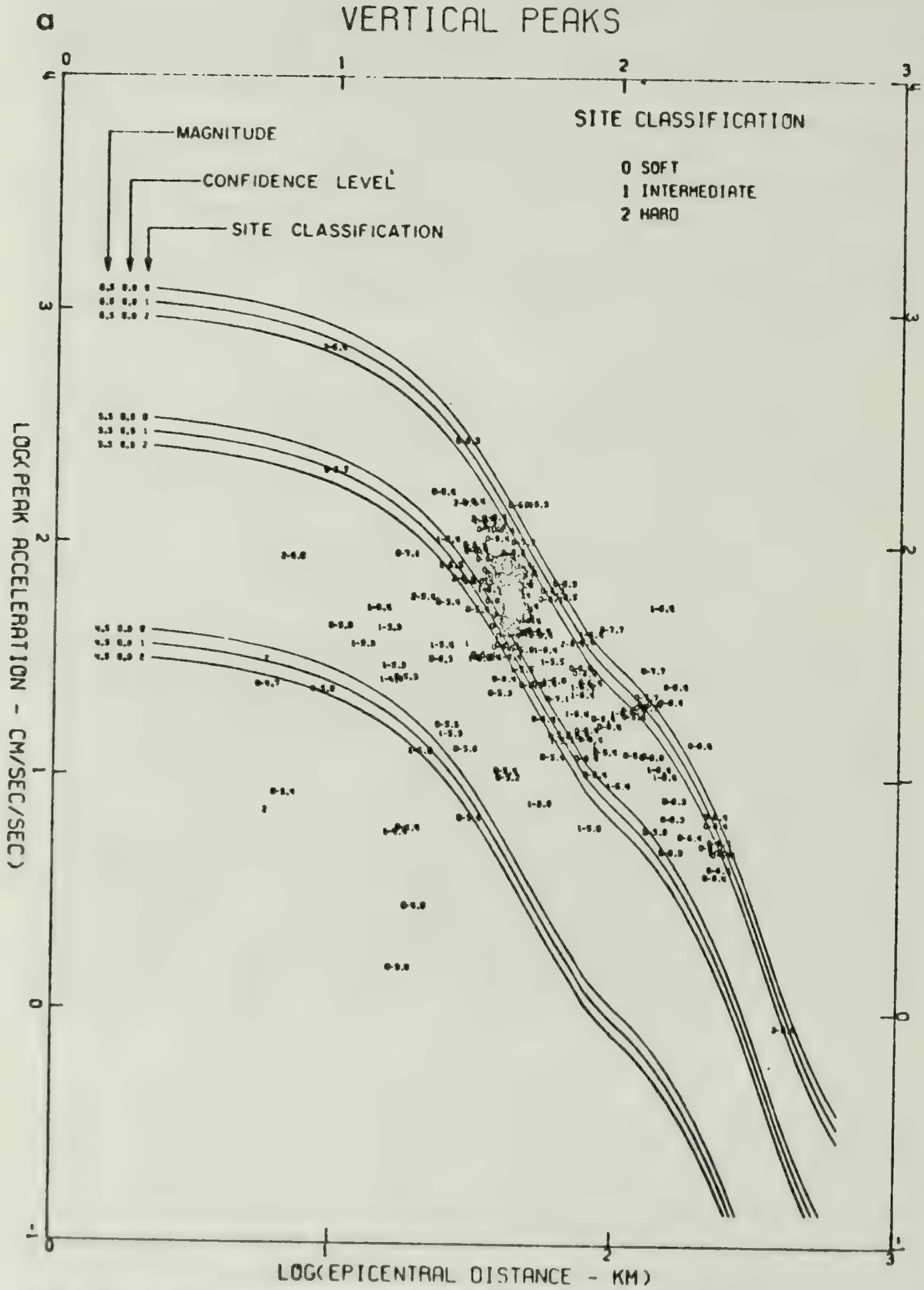
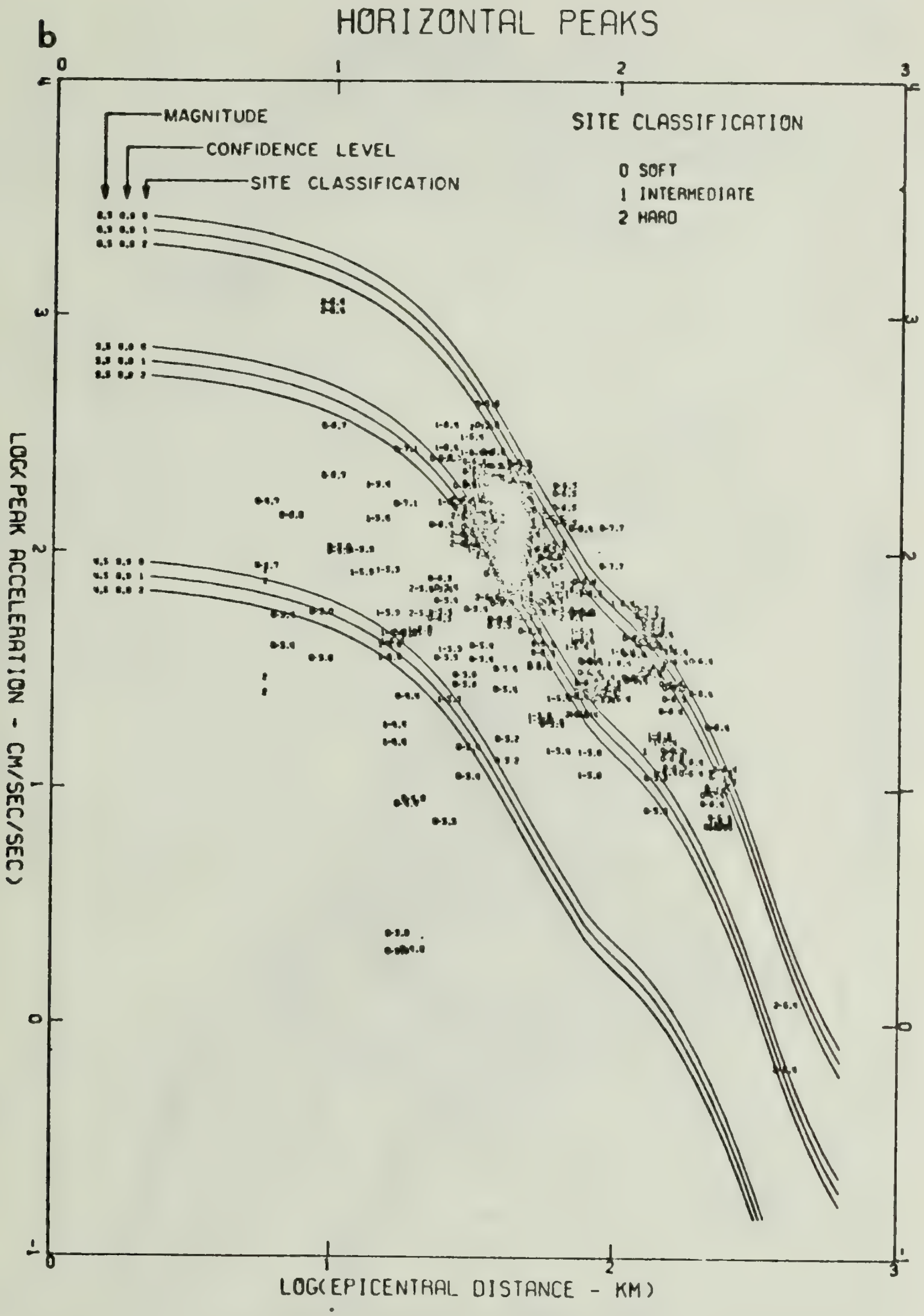


FIG. 1. Vertical and horizontal peak accelerations versus epicentral distance. Each plotted point has the site classification and magnitude, when available. Continuous lines represent the estimates of an upper bound on peak amplitudes and depend on earthquake magnitude, confidence level, and site classification.



The apparent frequency-dependent attenuation of wave amplitudes associated with frequency ω and often modeled approximately by $\exp[-(\omega R/2Q\beta)]$, where R may be taken to be epicentral distance, Q is the attenuation constant, and β is the shear-wave velocity, will also be neglected in this paper. This effect may be introduced into the analysis

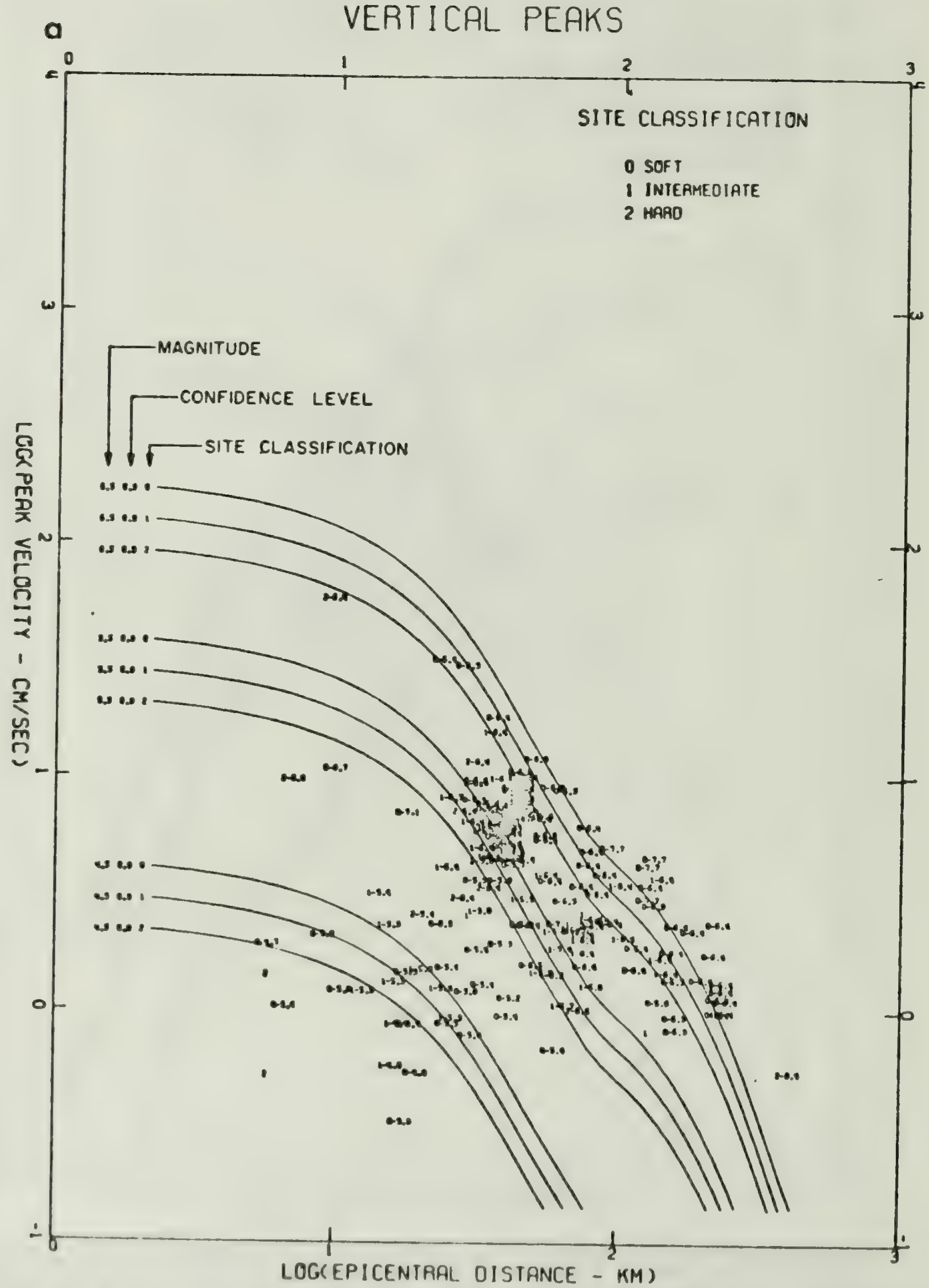
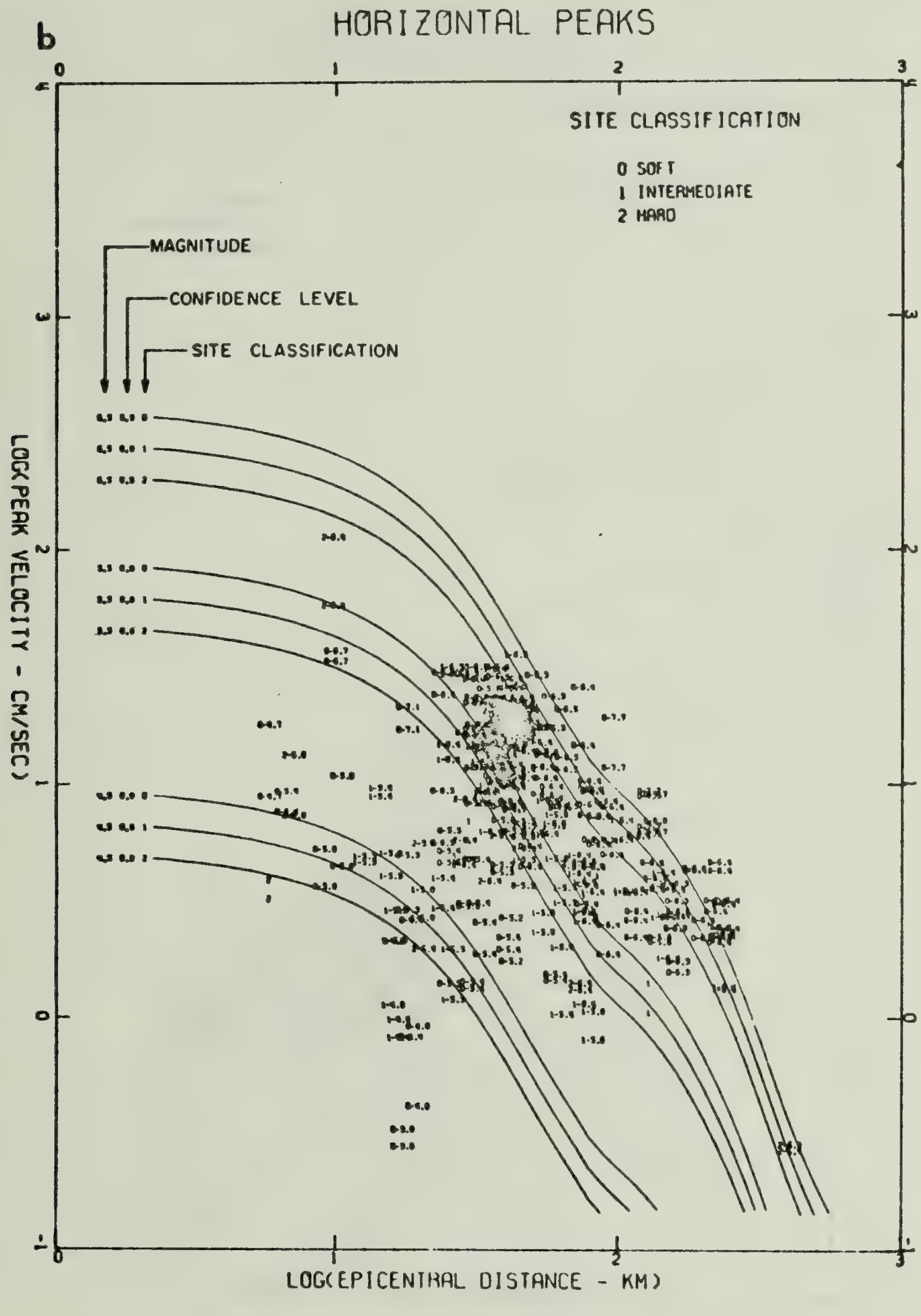


FIG. 2. Vertical and horizontal peak velocities versus epicentral distance. Each plotted point has the site classification and magnitude, when available. Continuous lines represent the estimates of an upper bound on peak amplitudes and depend on earthquake magnitude, confidence level, and site classification.



of spectral amplitudes but is difficult to incorporate into peak amplitude characterizations, since the representative frequency contents of peak amplitudes change with distance and because the relative contribution of digitization noise (Trifunac and Lee, 1974) varies with frequency and distance.

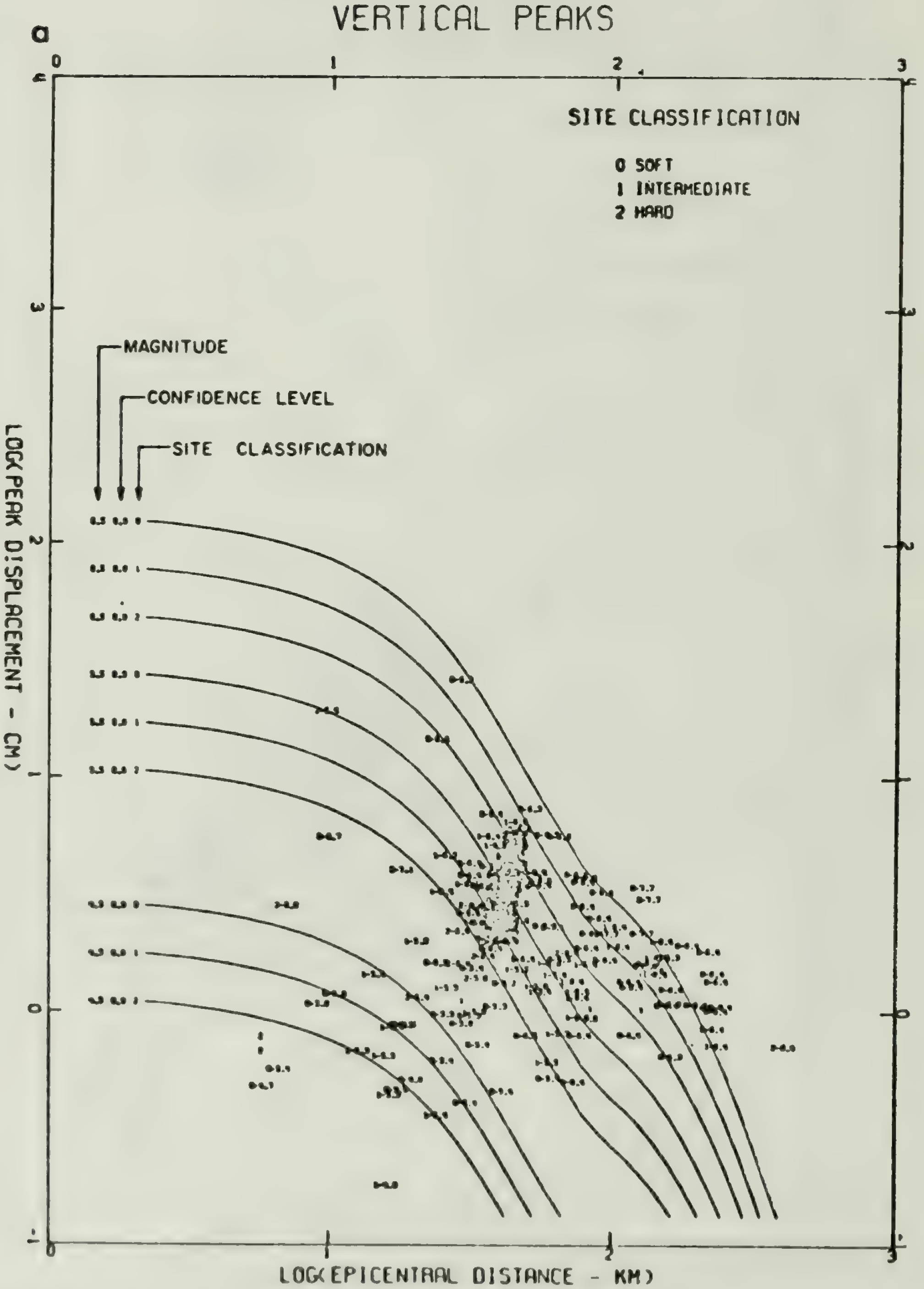
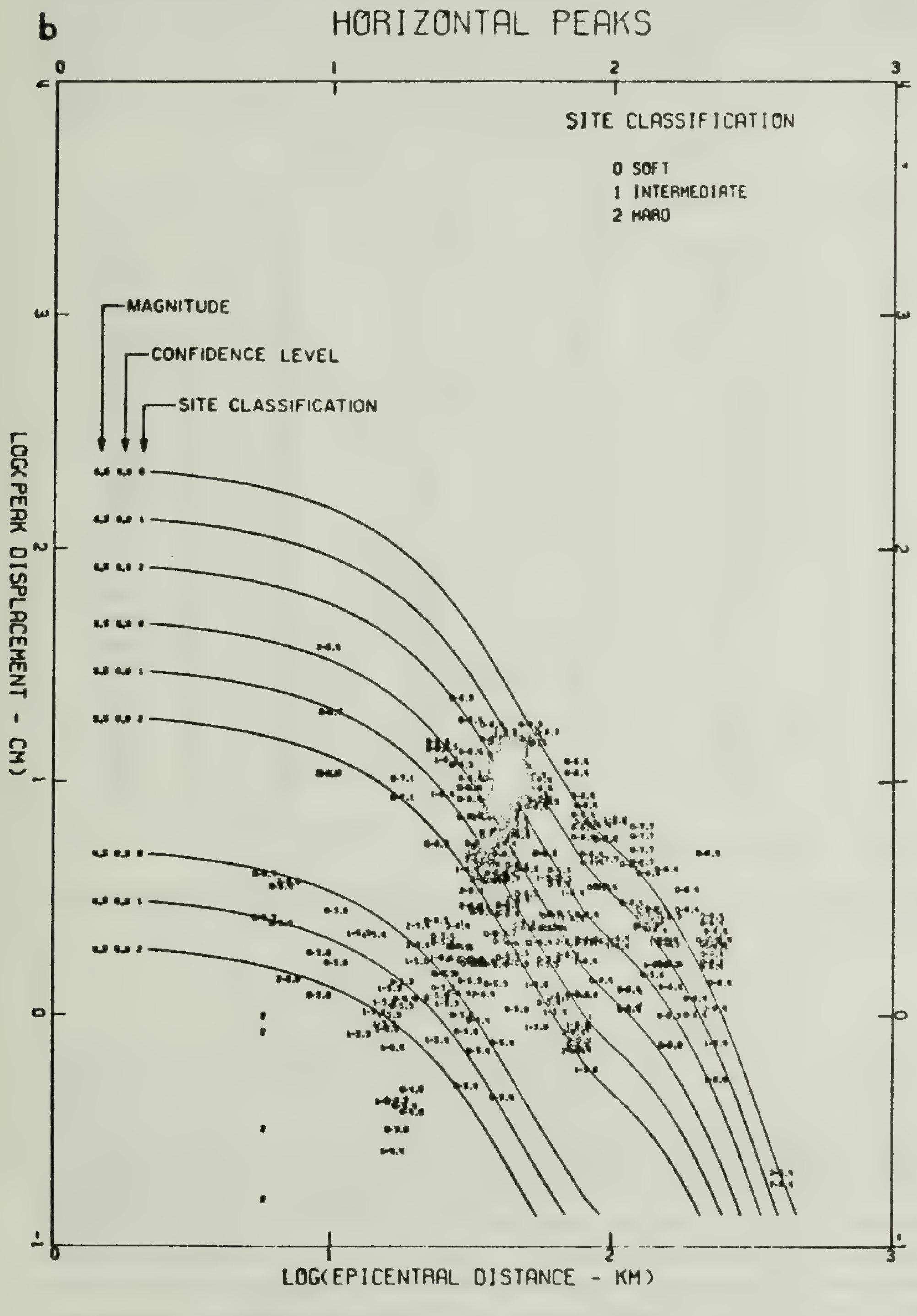


FIG. 3. Vertical and horizontal peak displacements versus epicentral distance. Each plotted point has the site classification and magnitude, when available. Continuous lines represent the estimates of an upper bound on peak amplitudes and depend on earthquake magnitude, confidence level, and site classification.

b



In this study the numerical values of $\log_{10} A_0(R)$ are taken to be those given by Richter (1958), and the appropriate scaling factors are included in $a_0(M)$, $v_0(M)$, and $d_0(M)$. The physical significance of $\log_{10} A_0(R)$ for our present work lies in its relative

changes of amplitude with distance which can be characterized by defining a new function $f(R)$ which is given by

$$f(R) = \log_{10} A_0(R = 0) - \log_{10} A_0(R). \tag{2}$$

TABLE 1

$\log_{10} A_0(R)$ VERSUS EPICENTRAL DISTANCE R*

R (km)	$-\log_{10} A_0(R)$	R (km)	$-\log_{10} A_0(R)$	R (km)	$-\log_{10} A_0(R)$
0	1.400	140	3.230	370	4.336
5	1.500	150	3.279	380	4.376
10	1.605	160	3.328	390	4.414
15	1.716	170	3.378	400	4.451
20	1.833	180	3.429	410	4.485
25	1.955	190	3.480	420	4.518
30	2.078	200	3.530	430	4.549
35	2.199	210	3.581	440	4.579
40	2.314	220	3.631	450	4.607
45	2.421	230	3.680	460	4.634
50	2.517	240	3.729	470	4.660
55	2.603	250	3.779	480	4.685
60	2.679	260	3.827	490	4.709
65	2.746	270	3.877	500	4.732
70	2.805	280	3.926	510	4.755
80	2.920	290	3.975	520	4.776
85	2.958	300	4.024	530	4.797
90	2.989	310	4.072	540	4.817
95	3.020	320	4.119	550	4.835
100	3.044	330	4.164	560	4.853
110	3.089	340	4.209	570	4.869
120	3.135	350	4.253	580	4.885
130	3.182	360	4.295	590	4.900

* Only the first two digits may be assumed to be significant.

Figure 4 shows a plot of $f(R)$ versus epicentral distance, R, and how it can be approximated by two straight line segments which are given by

$$f(R) = \begin{cases} R/50 & \text{for } R \leqq 75 \text{ km} \\ 1.125 + R/200 & \text{for } 350 \geqq R \geqq 75 \text{ km} \end{cases} \tag{3}$$

The change of slope at $R = 75$ km reflects the fact that for greater distances the main contribution to strong shaking comes from surface waves, which are attenuated less rapidly ($\sim 1/R^{\frac{1}{2}}$) than the near-field and intermediate-field ($\sim 1/R^{2-4}$), or far-field body waves ($\sim 1/R$). The function $f(R)$ in Figure 4 has been derived from Table 1 which represents a smoothed version of a similar table presented by Richter (1958). The third and fourth decimal places in this table have no significance and are kept only to preserve the smoothness of $A_0(R)$ amplitudes when plotted versus R. The reliability of shape and amplitudes of $\log_{10} A_0(R)$, i.e. $f(R)$, curves for epicentral distances less than about 10 km is not known, since at these short distances standard Wood-Anderson instruments go off scale for moderate and large earthquakes and the adequate number of strong-motion recordings is completely lacking there as well.

It is important to note here that $\log_{10} A_0(R)$ is of special value for the scaling functions studied in this paper because it incorporates empirically the average amplitude attenuation with distance in the Southern California Region and thus experimentally includes

the average properties of the Earth's crust in this area. Since most strong-motion data have been recorded in the same area, this curve represents the most natural first approximation to be used for scaling the strong-motion data as well. While it appears that this amplitude attenuation law might also be applied in other parts of the Western United States, it cannot be used for Central and Eastern United States (Nuttli, 1973).

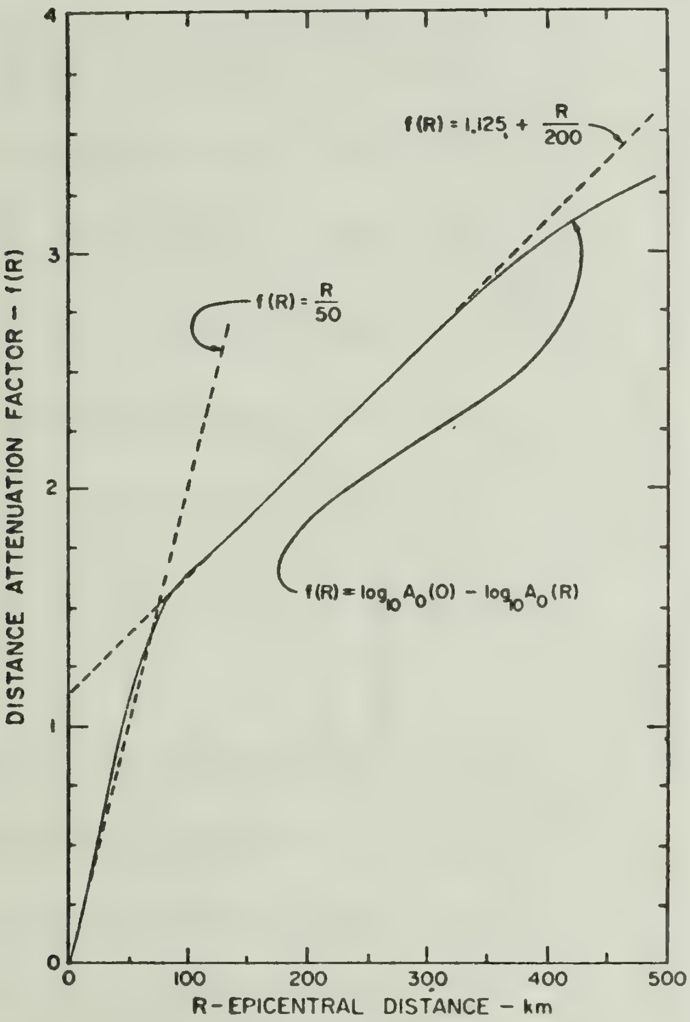


FIG. 4. Distance attenuation factor $f(R)$.

To extend the applicability of equation (1) so that it can be used for approximate scaling of the peaks of strong ground motion when the confidence with which such an estimate is made has been specified one can write

$$\log_{10}\begin{Bmatrix} a_{\max,p} \\ v_{\max,p} \\ d_{\max,p} \end{Bmatrix} = M + \log_{10} A_0(R) - \log_{10}\begin{Bmatrix} a_0(M,p,s,v) \\ v_0(M,p,s,v) \\ d_0(M,p,s,v) \end{Bmatrix}, \tag{4}$$

where M is earthquake magnitude; p is the confidence level associated with the approximate bounds $a_{\max,p}$, $v_{\max,p}$, and $d_{\max,p}$ for the peaks $a_{\max}$, $v_{\max}$, and $d_{\max}$; s represents the type of site conditions ($s = 0$ for alluvium deposits; $s = 1$ for "intermediate" rock;

TABLE 2

A Compressed Version of Data Tabulated for the Four Magnitude Intervals

$$\text{Amplitudes of } \log_{10}\begin{pmatrix} a_0(M, p, s, v) \\ v_0(M, p, s, v) \\ d_0(M, p, s, v) \end{pmatrix} = M + \log_{10} A_0(R) - \log_{10}\begin{pmatrix} a_{\max, p} \\ v_{\max, p} \\ d_{\max, p} \end{pmatrix}$$

Confidence Level p	Magnitude Range 4.0–4.9				Magnitude Range 5.0–5.9						Magnitude Range 6.0–6.9						Magnitude Range 7.0–7.9	
	Site class. $s = 0$		Site class. $s = 1$		Site class. $s = 0$		Site class $s = 1$		Site class. $s = 2$		Site class. $s = 0$		Site class. $s = 1$		Site class. $s = 2$		Site class. $s = 0$	
	vert. $v = 1$	hor. $v = 0$	vert. $v = 1$	hor. $v = 0$	vert. $v = 1$	hor. $v = 0$	vert. $v = 1$	hor. $v = 0$	vert. $v = 1$	hor. $v = 0$	vert. $v = 1$	hor. $v = 0$	vert. $v = 1$	hor. $v = 0$	vert. $v = 1$	hor. $v = 0$	vert. $v = 1$	hor. $v = 0$
									Acceleration $a_0(M, p, s, v)$									
0.10					2.55	2.20	2.38	1.98			2.54	2.22	2.56	2.18	2.73	2.37		3.14
0.15																	3.38	
0.20		1.89			2.46	2.10	2.20	1.92			2.39	2.14	2.45	2.10	2.55	2.31		3.13
0.25				1.48						1.84								
0.30					2.10	1.79	2.10	1.76			2.34	2.08	2.43	2.04	2.52	2.30	3.31	2.95
0.35	1.84	1.69																
0.40					1.97	1.70	2.08	1.69			2.30	2.02	2.36	1.99	2.43	2.24		2.93
0.45																	3.26	
0.50		1.25	1.91	1.40	1.76	1.65	1.99	1.61	1.81	1.74	2.24	1.95	2.29	1.97	2.34	2.22		2.87
0.55																		
0.60					1.72	1.51	1.98	1.57			2.18	1.90	2.22	1.94	2.25	2.17	3.22	2.84
0.65																		
0.70	1.80	1.25			1.69	1.38	1.94	1.43			2.13	1.84	2.18	1.91	2.22	1.98		2.82
0.75				0.73						1.07							3.11	
0.80					1.53	1.16	1.86	1.34			2.04	1.71	2.11	1.85	2.14	1.88		2.80
0.85		1.24																
0.90					1.40	0.85	1.65	1.17			1.87	1.59	2.08	1.76	2.10	1.79	3.11	2.64

					Velocity $v_0(M, p, s, r)$													
0.10					3.85	3.20	3.57	3.25			3.44	3.13	3.60	3.26	3.92	3.72		4.12
0.15																	4.50	
0.20		2.71			3.50	3.01	3.46	3.04			3.37	3.02	3.51	3.12	3.72	3.64		4.11
0.25				2.76						3.29								
0.30					3.29	2.86	3.39	2.96			3.33	2.90	3.32	3.00	3.61	3.47	4.49	4.01
0.35	2.90	2.59																
0.40					3.15	2.81	3.28	2.89			3.14	2.84	3.27	2.92	3.54	3.40		3.78
0.45																	4.09	
0.50		2.24	2.73	2.68	3.10	2.70	3.21	2.76	3.17	2.84	3.09	2.79	3.26	2.85	3.41	3.29		3.74
0.55																		
0.60					2.95	2.52	3.11	2.73			3.04	2.72	3.18	2.82	3.37	3.17	4.08	3.68
0.65																		
0.70	2.68	2.24			2.78	2.42	2.99	2.63			2.95	2.68	3.16	2.77	3.35	3.08		3.62
0.75				2.23						2.33							4.05	
0.80					2.67	2.32	2.83	2.50			2.81	2.53	3.03	2.73	3.27	2.91		3.58
0.85		2.21																
0.90					2.37	2.06	2.66	2.44			2.72	2.36	2.97	2.64	3.04	2.60	3.95	3.55
					Displacement $d_0(M, p, s, r)$													
0.10					4.12	3.58	3.95	3.70			3.80	3.44	3.90	3.65	4.19	4.30		4.50
0.15																	4.73	
0.20		3.02			3.69	3.46	3.94	3.58			3.72	3.30	3.81	3.53	4.06	4.16		4.41
0.25				3.26						3.28								
0.30					3.59	3.34	3.78	3.48			3.65	3.21	3.67	3.42	4.01	4.02	4.62	4.33
0.35	3.51	2.77																
0.40					3.42	3.29	3.71	3.37			3.43	3.08	3.59	3.25	4.01	3.89		4.11
0.45																	4.42	
0.50		2.62	3.03	2.81	3.25	3.11	3.26	3.18	3.52	3.20	3.31	3.01	3.51	3.18	3.88	3.78		4.08
0.55																		
0.60					3.11	3.00	3.16	3.12			3.24	2.94	3.47	3.07	3.81	3.69	4.23	4.07
0.65																		
0.70	2.68	2.58			3.06	2.75	3.11	2.95			3.10	2.87	3.40	3.04	3.70	3.49		4.03
0.75				2.66						2.83							4.09	
0.80					2.93	2.61	2.97	2.81			2.97	2.68	3.22	2.98	3.62	3.24		3.91
0.85		2.56																
0.90					2.78	2.24	2.89	2.72			2.80	2.51	3.05	2.86	3.52	2.76	4.06	3.86
No. of Data	3	6	2	4	24	47	15	30	2	4	82	164	34	68	12	24	7	14

$s = 2$ for basement rock); and r is used to describe the component direction ($r = 0$ for horizontal and $r = 1$ for vertical direction). As an approximation, we assume that the scaling functions $a_0(M, p, s, r)$, $r_0(M, p, s, r)$, and $d_0(M, p, s, r)$ can be described by

$$\log_{10}\begin{Bmatrix} a_0(M, p, s, r) \\ r_0(M, p, s, r) \\ d_0(M, p, s, r) \end{Bmatrix} = ap + bM + c + ds + er + fM^2 \tag{5}$$

where a, b, c, d, e, and f are the coefficients which have to be determined. In this paper we shall neglect the second and higher order terms of p and s and the third and higher order terms of M, as well as the terms which include different products of p, s and M. The data we have at our disposal now are not uniformly representative for different values of the parameters s and M (e.g., 63 per cent of all data have been classified as $s = 0$ and only 8 per cent as $s = 2$, while 71 per cent of all data belong to the magnitude range from 6.0 to 6.9) so that the estimates of the coefficients of the higher order terms than those in equation (5) might be affected by this nonuniformity of the data rather than being representative of the real trends of the scaling functions a_0, r_0 and d_0.

Regression Analysis

To compute the coefficients a, b, c, d, e, and f in equation (5), it is necessary to determine the estimates of $\log_{10}[a_0(M, p, s, r)]$, $\log_{10}[r_0(M, p, s, r)]$, and $\log_{10}[d_0(M, p, s, r)]$ for different values of their arguments by using the available strong-motion data. Since there are only 181 strong-motion accelerograms that can be subdivided into the needed subgroups, it is clear that the presently available data are far from adequate to characterize the $\log_{10}[a_0(M, p, s, r)]$, $\log_{10}[r_0(M, p, s, r)]$, and $\log_{10}[d_0(M, p, s, r)]$ over a sufficiently broad range of their arguments (note that 181 rather than 187 accelerograms have been used because of incomplete availability of magnitude determinations). Nevertheless, an attempt can be made to determine a first approximation to the coefficients a, b, c, d, e and f. To do this we begin by partitioning all data into four groups that correspond to the magnitude ranges 4.0–4.9, 5.0–5.9, 6.0–6.9, and 7.0–7.9. Each of these groups is next subdivided into three subgroups which correspond to the site classifications, s ($s = 0$, 1 or 2). Depending on whether the recording component is horizontal or vertical, each of these subgroups is finally divided into two parts corresponding to $r = 0$ and $r = 1$. Within each of these parts, we calculate

$$\log_{10}\begin{Bmatrix} a_0(M, p, s, r) \\ r_0(M, p, s, r) \\ d_0(M, p, s, r) \end{Bmatrix} = M + \log_{10} A_0(R) - \log_{10}\begin{Bmatrix} a_{max} \\ r_{max} \\ d_{max} \end{Bmatrix}. \tag{6}$$

An array of n data points in one such part can be further arranged so that the numerical values of $\log_{10}[a_0(M, p, s, r)]$, $\log_{10}[r_0(M, p, s, r)]$, and $\log_{10}[d_0(M, p, s, r)]$ decrease monotonically with increasing n. Then, if m = integer part of (pn), the mth data point will represent an estimate for an upper bound of $\log_{10}[a_0(M, p, s, r)]$, $\log_{10}[r_0(M, p, s, r)]$, or $\log_{10}[d_0(M, p, s, r)]$, which is associated with the p-per cent confidence level. Table 2 presents a compressed version of such data tabulated for the four magnitude intervals, and indicates the number of data points used in their estimates. For actual calculations we used the confidence levels equal to 0.5, 0.10, 0.15, . . ., 0.85, 0.90, and 0.95 and the reported magnitudes corresponding to each individual estimate of $\log_{10}[a_0(M, p, s, r)]$, $\log_{10}[r_0(M, p, s, r)]$, and $\log_{10}[d_0(M, p, s, r)]$ whenever the number of data points within each part permitted such detailed classification.

Table 3 presents the results of least-squares fitting of equations (5) to the above data. It presents the estimates of the coefficients $a, b, \ldots, e$, and f; the total number of data points which have been used in the fitting; and the $M_{\min}$ and $M_{\max}$, which are the lowest and the highest magnitudes for which equations (5) are assumed to apply.

The curves $\log_{10}[a_0(M, p, s, v)]$, $\log_{10}[v_0(M, p, s, v)]$, and $\log_{10}[d_0(M, p, s, v)]$ represent parabolas when plotted versus M. Their amplitudes depend on p, s, and v. As far as their functional form is concerned, it should be noted here, however, that there is no direct physical justification for such parabolic dependence, and that this choice is motivated by the simplicity of its mathematical form and the apparent trend of data indicated by our previous analyses (Trifunac and Brady, 1975b). Since this overall trend of data suggests that the amplitudes of $\log_{10}[a_0(M, p, s, v)]$, $\log_{10}[v_0(M, p, s, v)]$, and $\log_{10}[d_0(M, p, s, v)]$ might level off for some small magnitude, we have decided, quite

TABLE 3

COEFFICIENTS IN THE EXPRESSION*

$$\log_{10}\begin{Bmatrix} a_0(M, p, s, v) \\ v_0(M, p, s, v) \\ d_0(M, p, s, v) \end{Bmatrix} = \begin{cases} ap + bM + c + ds + ev + fM^2 - f(M - M_{\max})^2 & \text{for } M \geqq M_{\max} \\ ap + bM + c + ds + ev + fM^2 & \text{for } M_{\max} \geqq M \geqq M_{\min} \\ ap + bM_{\min} + c + ds + ev + fM^2_{\min} & \text{for } M \leqq M_{\min} \end{cases}$$

Function	a	b	c	d	e	f	N Data	$M_{\min}$	$M_{\max}$
$\log_{10} a_0(M, p, s, v)$	−0.898	−1.789	6.217	0.060	0.331	0.186	227	4.80	7.50
$\log_{10} v_0(M, p, s, v)$	−1.087	−2.059	8.357	0.134	0.344	0.201	227	5.12	7.61
$\log_{10} d_0(M, p, s, v)$	−1.288	−2.366	9.717	0.205	0.240	0.226	227	5.24	7.45

* Only the first two digits may be assumed to be significant.

arbitrarily, to adopt equations (5) for $M \geqq M_{\min}$, where $M_{\min}$ is that value of M for which the minima of the above functions are achieved and to use the constant amplitudes equal to these minima for $M \leqq M_{\min}$. $M_{\max}$ is defined as the point where the parabola reaches unit slope, equal to the slope of M in equation (4), and for $M \geqq M_{\max}$ the right-hand side of (5) continues linearly with this slope. This can be summarized as follows

$$\log_{10}\begin{Bmatrix} a_0(M, p, s, v) \\ v_0(M, p, s, v) \\ d_0(M, p, s, v) \end{Bmatrix} =$$

$$\begin{cases} ap + bM + c + ds + ev + fM^2 - f(M - M_{\max})^2 & \text{for } M \geqq M_{\max} \\ ap + bM + c + ds + ev + fM^2 & \text{for } M_{\max} \geqq M \geqq M_{\min} \\ ap + bM_{\min} + c + ds + ev + fM^2_{\min} & \text{for } M \leqq M_{\min} \end{cases} \quad (7)$$

and means that the maxima of $a_{\max}$, $v_{\max}$ and $d_{\max}$ are attained for $M \geqq M_{\max}$.

The values of the coefficients in equation (7) are shown in Table 3 where only the two first digits may be taken as significant. The confidence level coefficient "a" tends to increase slightly from acceleration to velocity and displacement indicating somewhat greater scatter of peak displacements, poorer fit of displacements to the $\log_{10} A_0(R)$ curve, a slight overestimate of maximum displacement peaks for large epicentral distances caused by digitization processing noise (computed displacement peaks which are equal to or less than several centimeters in most cases are contaminated by noise, Trifunac and Lee, 1974), or, most probably, some combination of all these effects. The values of site

coefficient "d" approximately double from acceleration to velocity and from velocity to displacement, reflecting progressively greater importance of site conditions on peak amplitudes of longer period waves. The same effect is also illustrated in Figures 1, 2, and 3 where the continuous confidence levels for $M = 4.5$, 5.5, and 6.5 for $p = 0.9$ and for $s = 0$, 1 and 2 which are calculated from the coefficients in Table 3 have been presented. These results thus indicate that for a given confidence level the peak acceleration is influenced only to a small degree by the site conditions and that it is about 30 per cent larger for alluvium site conditions ($s = 0$) than for rock sites ($s = 2$). The corresponding difference for peak displacements, however, is equal to 250 per cent.

In our previous work dealing with the correlations of peak accelerations with Modified Mercalli Intensity (Trifunac and Brady, 1975a) or with the correlations of peak accelerations with earthquake magnitude and epicentral distance for moderate and small magnitudes (Trifunac and Brady, 1975b), we found that the different site conditions had no significant effect on the recorded peaks but that there exists a tendency for a slight, yet consistent, increase of peak amplitudes for harder sites ($s = 2$). These investigations deal with the amplitudes of the expected peaks, while the present paper investigates the approximate bounds of peak amplitudes. In view of the fact that we do expect to find the largest standard deviations for the peak amplitudes recorded on alluvium sites and because we employ $\log_{10} A_0(R)$ which does not depend on site conditions, it is not surprising that in this approximate analysis we find the overall bounds for peak accelerations to be slightly higher on alluvium than on hard-rock sites. Simple, two-dimensional theoretical studies, which are based on linear models, for example, show that the average peak accelerations on alluvium do not change much but that the variations from the mean grow rapidly with a decrease of wavelength and an increase in velocity contrast across a material discontinuity (e.g., Trifunac, 1971; Wong and Trifunac, 1974). Nevertheless, when more records become available at small distances from faults, we will most probably find that in the near-field the high-frequency large peak amplitudes will tend to be smaller on alluvium ($s = 0$) than on hard-rock sites ($s = 2$) because the peak amplitudes

TABLE 4

CHECKING OF THE SUCCESS IN PEAK BRACKETING

Confidence Level	Found Peaks Less than the Limit		Found Peaks Greater than the Limit	
	No.	Fraction	No.	Fraction
$p = 0.9$	1566	0.930	117	0.070
$p = 0.8$	1485	0.881	198	0.119
$p = 0.7$	1345	0.800	338	0.200
$p = 0.6$	1172	0.696	511	0.314
p 0.5	976	0.578	707	0.422
$p = 0.4$	743	0.440	940	0.560
$p = 0.3$	505	0.300	1178	0.700
$p = 0.2$	327	0.194	1356	0.806
$p = 0.1$	215	0.127	1468	0.873

Total no. of peaks = 1683*

* Acceleration, velocity and displacement peaks were lumped together. One vertical and two horizontal peaks were also lumped together.

that can be transmitted through the alluvium and soil deposits will be limited by the nonlinear response of these materials.

The value of coefficient "c" in Table 3 is close to 0.3 and shows that $a_{max,p}$, $v_{max,p}$ and $d_{max,p}$ for horizontal peaks are about twice as large as the corresponding levels for vertical peaks. This is in fair agreement with the trends indicated by the average peak amplitudes (Trifunac and Brady, 1975b) in similar correlations with earthquake magnitude and epicentral distance and in related correlations with the Modified Mercalli Intensity Scale (Trifunac and Brady, 1975a).

The above description of $a_0(M, p, s, r)$, $v_0(M, p, s, r)$ and $d_0(M, p, s, r)$ involves several oversimplifying assumptions and should therefore be evaluated critically. To do this we calculated $a_{max,p}$, $v_{max,p}$ and $d_{max,p}$ for the parameters that correspond to those

TABLE 5

HORIZONTAL PEAK DISPLACEMENTS $\log_{10}[d_{max}(R = 0)]$ (d_{max} IS IN CENTIMETERS)*

Site Classification	$p = 0.5$	$p = 0.6$	$p = 0.7$	$p = 0.8$	$p = 0.9$
			$M = 7.5$		
0	2.06	2.19	2.32	2.45	2.58
1	1.86	1.98	2.11	2.24	2.37
2	1.65	1.78	1.91	2.04	2.17
			$M = 6.5$		
0	1.86	1.99	2.12	2.44	2.37
1	1.65	1.78	1.91	2.04	2.17
2	1.45	1.58	1.71	1.83	1.96
			$M = 5.5$		
0	1.20	1.33	1.46	1.59	1.72
1	1.00	1.13	1.26	1.38	1.51
2	0.79	0.92	1.05	1.18	1.31
			$M = 4.5$		
0	0.22	0.35	0.48	0.61	0.73
1	0.014	0.14	0.27	0.40	0.53
2	−0.19	−0.062	0.067	0.20	0.32

* Only the first two digits may be assumed to be significant.

for all components of recorded peak acceleration, velocity, and displacement and for nine confidence levels equal to 0.1, 0.2, . . ., 0.8, and 0.9. For every confidence level this amounts to comparing 1683 data points with the calculated bounds $a_{max,p}$, $v_{max,p}$, and $d_{max,p}$ and tabulation of the percentage of recorded peaks that are actually below or above the computed bounds. If one pools all peaks together, then this comparison is as indicated in Table 4. This table shows that in all but one case for $p = 0.2$ the number of peaks below the corresponding bound is equal to or greater than that required by the percentage equal to p. Consequently, it may be concluded from this test that the above simplifying assumptions are acceptable for derivation of approximate bounds of the currently available peaks of strong ground motion.

Tables 5, 6, and 7 and Figure 4 enable one to calculate quickly the logarithms for the approximate bounds on peak acceleration, velocity, and displacement by subtracting the

TABLE 6

HORIZONTAL PEAK VELOCITIES—$\log_{10}[v_{max}(R = 0)]$ (v_{max} IS IN CENTIMETERS PER SECOND)*

Site Classification	$p = 0.5$	$p = 0.6$	$p = 0.7$	$p = 0.8$	$p = 0.9$
			$M = 7.5$		
0	2.42	2.53	2.64	2.75	2.85
1	2.95	2.39	2.47	2.61	2.72
2	2.15	2.26	2.37	2.48	2.58
			$M = 6.5$		
0	2.18	2.29	2.40	2.50	2.61
1	2.04	2.15	2.26	2.37	2.48
2	1.91	2.02	2.13	2.24	2.34
			$M = 5.5$		
0	1.53	1.63	1.75	1.86	1.97
1	1.40	1.50	1.61	1.72	1.83
2	1.26	1.37	1.48	1.56	1.70
			$M = 4.5$		
0	0.560	0.668	0.777	0.885	0.994
1	0.425	0.534	0.643	0.751	0.860
2	0.291	0.400	0.509	0.618	0.726

* Only the first two digits may be assumed to be significant.

TABLE 7

HORIZONTAL PEAK ACCELERATIONS—$\log_{10}[a_{max}(R = 0)]$ (a_{max} IS IN CENTIMETERS PER SECOND PER SECOND)*

Site Classification	$p = 0.5$	$p = 0.6$	$p = 0.7$	$p = 0.8$	$p = 0.9$
			$M = 7.5$		
0	3.29	3.38	3.47	3.56	3.65
1	3.23	3.32	3.41	3.50	3.59
2	3.17	3.26	3.35	3.44	3.53
			$M = 6.5$		
0	3.10	3.19	3.28	3.37	3.46
1	3.04	3.13	3.22	3.31	3.40
2	2.98	3.07	3.16	3.25	3.34
			$M = 5.5$		
0	2.54	2.63	2.72	2.81	2.90
1	2.49	2.57	2.66	2.75	2.84
2	2.42	2.51	2.60	2.69	2.78
			$M = 4.5$		
0	1.63	1.72	1.81	1.90	1.99
1	1.57	1.66	1.75	1.84	1.93
2	1.51	1.60	1.69	1.78	1.87

* Only the first two digits may be assumed to be significant.

value for $f(R)$ at a selected distance from an appropriate entry in Tables 5, 6, or 7. Formally, Tables 5, 6, and 7 represent the estimates of bounds on maximum peak acceleration, velocity, and displacement at $R = 0$. However, as we already pointed out, since the adequacy of $\log_{10} A_0(R)$ for R less than 10 to 20 km cannot be tested critically because there is virtually no strong-motion data for these short distances, the entries in Tables 5, 6, and 7 can, so far, only be interpreted to represent the scaling factors for use with $f(R)$ and are valid for approximate scaling of peaks only for R between about 20 and 200 km.

Comparison of the Empirically Determined Peak Amplitudes with those Derived from Source Mechanism Studies

In the above analysis we have presented scaling relationships for peaks of acceleration, velocity, and displacement for the assumed empirical law of attenuation versus distance given by $\log_{10} A_0(R)$ (Richter, 1958). As shown in the previous study (Trifunac and Brady, 1975b), this attenuation law represents an acceptable approximation to the observed amplitude variations with distance for epicentral distances ranging from about 20 km to about 200 km. Although Richter (1958) presents the curve $\log_{10} A_0(R)$ for distances ranging from 0 km to well over 600 km, at the present time there is not enough recorded strong-motion data for distances less than 20 km or greater than 200 km to test whether this same attenuation law can be extended outside the range for which we have already suggested its approximate validity. Our next aim is, therefore, to examine whether the extrapolation is permissible and under what conditions, if any, the above-developed scaling laws may be used for the interim prediction of peaks at distances which are less than 20 km before more accurate attenuation laws become available. Testing of such an extension is clearly more important for distance less than 20 km than it is at distances greater than 200 km because there is a lack of recorded data at small distances where the peak accelerations will be largest.

In the following three sections we make an attempt to examine the plausibility of extrapolating the above-developed scaling laws back to zero epicentral distance. We do this by comparing the peaks predicted by our present analysis with other independent calculations derived from several source mechanism studies which were carried out in the same geographic area in which the strong-motion accelerograms have been recorded.

In all calculations in this paper we use epicentral rather than hypocentral or closest distance to the fault. While this is a necessary simplification, since the fault depths and orientations are accurately known for only a few earthquakes studied in this paper, the flattening nature of $\log_{10} A_0(R)$ for R small and the fact that most earthquakes in California have hypocentral depth less than 15 km seem to justify this approximation. In any case, peak amplitudes of the near-field strong ground motion seem to result from localized and energetic motions somewhere on the fault. Since there is no reason to believe that the fault section contributing most to these peak amplitudes is located at the focus or at the point which is closest to the recording station, even if we knew the hypocentral or the distance perpendicular to the fault, it would still be difficult to justify the choice of either of these two distances as being significantly better for the present application than the epicentral distance. This is, of course, correct only if the source dimension is not much larger than the epicentral distance. For large shallow earthquakes which may be characterized by long faults the distance perpendicular to the fault would seem to be most appropriate if the fault-to-station distance is much smaller than the epicentral distance. However, since no earthquake studied in this paper unequivocally falls in this category, we chose to work with the epicentral distance only.

(*a*) *Peaks of strong-motion displacement.* Table 5 summarizes the estimates of maxi-

mum peak displacements for several different magnitudes, M, confidence levels, p, and site classifications, $s = 0, 1$, and 2. The amplitudes in this table have been derived by extrapolating from the distance range to which equation (4) applies to the epicentral distance $R = 0$.

For shallow or surface faults the maximum dynamic displacement amplitude of strong ground motion, d_{max}, would be expected to approach one-half of the maximum static dislocation amplitude, u_{max}, as $R \to 0$ if we assume that no significant overshoot of dislocation takes place. For a number of simple fault geometries (see references in Table 1 of Trifunac, 1973) the maximum static dislocation amplitude, u_{max}, and the overall average static dislocation amplitude, $\bar{u}$, are approximately related by

$$\bar{u} \approx \frac{3}{4} u_{max}. \tag{8}$$

Consequently, we have

$$\bar{u} \approx \frac{3}{2} d_{max}. \tag{9}$$

The plausibility of this statement may be based on the fact that the frequencies associated with ground displacement are low and that the main contribution to the displacement amplitudes for small R comes from the near-field terms in the DeHoop's Representation Theorem (Haskell, 1969) in which the relative motion on the fault dominates the characteristics of displacements in the vicinity of the fault. On the other hand, the weakness of this statement is related to the fact that the "static" fault displacement, $\bar{u}$, is being correlated with the "observed dynamic" peak displacement from which all the periods longer than about 15 sec have been filtered out (Trifunac and Lee, 1974). Consequently, in the near-field and for large earthquakes the "observed dynamic" peak displacement, d_{max}, may be smaller than the actual peak displacement because of the limitations imposed by the currently available methods for double integration of recorded accelerograms (Trifunac and Lee, 1974).

Source mechanism studies based on spectra of recorded P and S waves (e.g., Brune, 1970; Hanks and Wyss, 1972; Trifunac, 1972a, 1972b) usually characterize an earthquake source in terms of two independent static parameters which are often selected to be: (1) Seismic moment, M_0, and (2) source dimension, r. The seismic moment is defined by

$$M_0 = \mu \bar{u} A, \tag{10}$$

where μ is the rigidity constant in the source region, $\bar{u}$ is the average dislocation amplitude, and A is the area of fault. Assuming that the fault area can be approximated by a disk of radius r, i.e. $A = \pi r^2$, from a known M_0 and r, one can calculate an estimate of the average dislocation $\bar{u}$ as follows

$$\bar{u} = \frac{M_0}{\mu \pi r^2}. \tag{11}$$

This result can be employed for comparison of the expected static displacements, $\bar{u}$, in the source region with the estimates of $\bar{u}$, using equation (9), compiled from dynamic displacement maxima which are calculated from strong-motion data at distance and by extrapolating back to $R = 0$ on the basis of equation (4) an Tables 1 and 3.

Figure 5 presents such a comparison for an average dislocation amplitude, $\bar{u}$, plotted versus local magnitude, M_L. The data representing $\bar{u}$ have been calculated from Table 1 of Thatcher and Hanks (1973) and by using equation (11). Additional data derived from the

source mechanism studies based on strong-motion accelerograms have been derived from Tables 1 and 3 of Trifunac (1972a) and Tables 1 and 2 of Trifunac (1972b). It can be seen from this figure that, although the data scatter is considerable, there is a general trend for dislocation amplitudes to increase for larger earthquake magnitudes.

Continuous curves in Figure 5 represent the amplitudes of $\bar{u}$ computed from equation (9) for $d_{max,p}$ with the confidence levels $p = 0.5$ and 0.9 and for the two site classifications $s = 0$ and $s = 2$. By comparing the overall trend of $\bar{u}$ data with these continuous lines, we find that they are in good agreement considering the number of uncertainties in the analysis and the simple approximations employed.

For the computation of $\bar{u}$ from the data of Thatcher and Hanks (1973), we used $\mu = 3 \times 10^{11}$ dyne/cm^2 in equation (11) and made no attempt to use lower values of μ for shallow or surface sources. More detailed analyses and classifications of Thatcher and

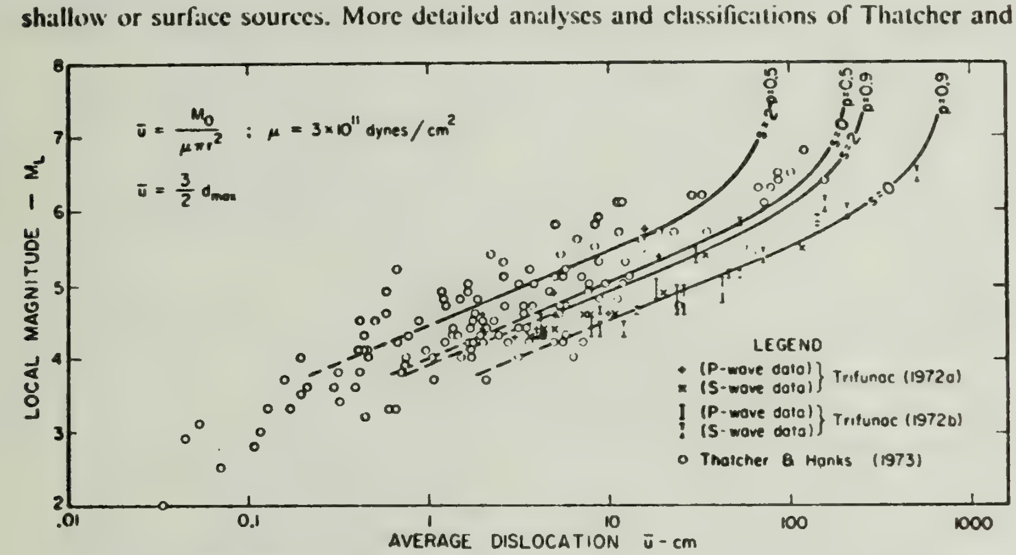


FIG. 5. Comparison of average dislocation amplitudes, $\bar{u}$, derived from three source mechanism studies (points) with amplitudes computed from statistical analysis of strong-motion data (lines).

Hanks' (1973) data, as well as our results based on strong-motion data (Trifunac, 1972a, 1972b), might reduce the overall scatter of points in Figure 5. Nevertheless, the agreement between the approximate inferences based on equations (9) and (11) and the analysis of peak amplitudes at $R = 0$ appears to be good. We interpret this agreement to mean that the empirical scaling and attenuation laws for peak displacements incorporated into equation (4) can be extended to apply for epicentral distances between 0 and 20 km.

(*b*) *Peaks of strong-motion velocity*. To test the amplitudes of peak velocities computed from equation (4) for epicentral distance $R = 0$, one may choose to work with another quantity which can be directly related to peak velocity, i.e., effective stress σ (Brune, 1970). This approach seems to represent the only alternative at this time because most observational source mechanism studies tabulate only the effective stress or stress drop as one of the basic parameters which could be related to the particle velocity at source (e.g., Brune, 1970; Hanks and Wyss, 1972; Thatcher and Hanks, 1973; Trifunac, 1972a, 1972b).

Using a one-dimensional analog of faulting in an infinite homogeneous space, Brune (1970) showed that the peak particle velocity in the near-field, v, the effective stress, σ (effective stress is the difference between initial static stress and frictional shear stress on the fault during rupture. A different kind of "stress drop" is represented by the difference

between the initial and final static stresses. For simplicity in this paper, these two different quantities will often be referred to simply as "stress drop"), the shear-wave velocity, β, and the material rigidity in the source region, μ, are all approximately related by

$$v(R = 0) \approx \sigma\beta/\mu. \tag{12}$$

If one estimates β and μ and assumes that $v \approx v_{max}$, where we are not including a correction for the reflection off the half-space since, at $R = 0$, v_{max} is the peak velocity recorded at the shallow or surface fault, then

$$\sigma \approx v_{max}(R = 0)\,\mu/\beta. \tag{13}$$

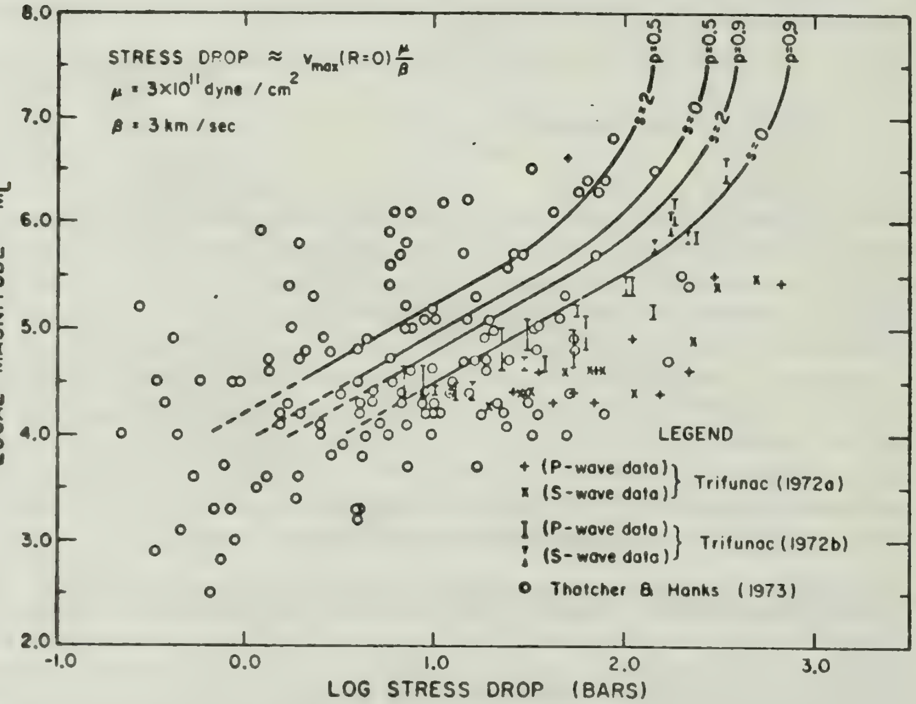


FIG. 6. Comparison of the estimates of stress drop computed in three source mechanism studies (points) with the estimates based on statistical analysis of strong-motion data (lines).

For $\beta \sim 3$ km/sec and $\mu = 10^{11}$ dynes/cm^2 to 3×10^{11} dynes/cm^2, equation (12) gives 1 to 3 cm/sec per one bar of stress drop. This range of peak amplitudes appears to be in good agreement with the work of Dietrich (1973) (Figure 5 in his paper) who parametrized peak velocity and distance by using stress drop and minimum rupture dimension in his finite element modeling of near-field ground motion.

With values of $\mu = 3 \times 10^{11}$ dynes/cm^2 and $\beta = 3$ km/sec, equations (4) and (13) can be combined to plot the corresponding estimates of confidence levels for the stress drop versus local earthquake magnitude M_L. Such plots are presented in Figure 6 for site classifications $s = 0$ and $s = 2$ and for two confidence levels $p = 0.5$ and $p = 0.9$. Here again, we are not using the lower values of μ for shallow sources, because for most data points in Figure 6 which are derived from the studies of Thatcher and Hanks (1973) and Trifunac (1972a, b) the source geology is not known in detail and because these investigators used $\mu = 3 \cdot 10^{11}$ dynes/cm^2 for the majority of their calculations. Since μ enters as a scaling constant in equations (11) and (13) in the same manner for both the source

mechanism studies as well as for the scaling laws studied in this paper, so long as we use the same numerical values in both calculations, the relative comparisons of the two will not be affected.

In addition to the estimates of stress drop from equations (4) and (13), Figure 6 presents the data on stress drop versus magnitude, M_L, derived from the studies by Thatcher and Hanks (1973), Trifunac (1972a), and Trifunac (1972b). All of these data have been computed by using the approximate spectral theory proposed by Brune (1970) and represent the stress drop or the effective stress that can be computed from the high-frequency amplitudes of *P*- and *S*-wave displacement spectra. For this reason such estimates represent, at best, only an overall average of what may be a rapidly varying stress over the fault plane and consequently may be subject to large fluctuations. For example, for aftershocks of a large earthquake, one might expect to find small source dimensions and high stress drops. This could be exemplified by the selected aftershock data we investigated for the Imperial Valley, California, earthquake of 1940 (Trifunac, 1972b) and the San Fernando, California, earthquake of 1971 (Trifunac, 1972a; Trifunac, 1974).

As indicated by the trend of data in Figure 6, for larger earthquakes (larger M_L), a larger overall average stress could be released. However, the spread of the estimated stress-drop data for a given magnitude, M_L, appears to be about three orders of magnitude.

From the point of view of prediction of amplitudes of strong earthquake ground motion in earthquake engineering applications, it may not always be feasible to devise a simple method for estimating the possible stress drop for a given earthquake source. Consequently, at the present time, in virtually all estimations of the amplitudes of strong ground motion, the stress drop is overlooked and only one-parameter-scaling in terms of earthquake magnitude is being employed. Such simplification can be justified from the practical point of view, since the data and statistical analyses of earthquake magnitude are more complete and reliable than the interpretations of the inferred amplitudes of stress drop. For simplicity in presentation and in complying with the practical constraints in applications, in this paper we also use only the rough approximate theory which is based on one-parameter-scaling in terms of earthquake magnitude.

In the light of the above-mentioned simple assumptions and approximations, we feel that the stress estimates based on equations (4) and (13) and those derived from three independent source mechanism studies (Figures 6) are not in contradiction. However, due to the large scatter of stress drop data, it should be pointed out that the amplitudes of the peaks predicted by equation (4) could be uncertain by as much as a factor of about 2 to 3, even if one were to assume that the estimates of stress drop in Figure 6 and their scatter reflect the variability of stress drop rather than the uncertainties in the computations which are based on the approximate Brune's (1970) theory.

(*c*) *Peaks of strong-motion acceleration.* The largest apparent peak of strong-motion acceleration that might be recorded at the source of earthquake energy release ($R = 0$) seems to depend most prominently on the effective stress and the recording instrument employed to record it. The effective stress is believed to be proportional to the high-frequency amplitude of the Fourier transform of ground accelerations at the fault (Brune, 1970), while the recording instrument, which is usually a single-degree-of-freedom oscillator with damping close to 50 per cent of critical, represents a low-pass filter which attenuates frequencies higher than its natural frequency, ω_n, in a manner proportional to $(\omega/\omega_n)^2$. Many other parameters, no doubt, influence the largest amplitude of accelerations at the fault but will be neglected in this approximate analysis in which we only consider the simplest available model (Brune, 1970) which can readily be correlated with the apparent characteristics of the near-field and far-field observations.

The amplitude of the Fourier transform $F_{NF}(\omega)$, of particle accelerations at the fault surface can be shown to be approximately given by

$$\underset{\omega \to \infty}{F_{NF}(\omega)} = \frac{\sigma\beta}{\mu} \frac{\omega}{(\omega^2 + \omega_\tau^2)^{\frac{1}{2}}} \tag{14}$$

(Brune, 1970), where $F_{NF}(\omega)$ is the Fourier transform of the absolute S-wave type accelerations, σ is the effective stress, β is the shear-wave velocity, and μ is the medium rigidity. For intermediate and low frequencies [$\omega \leqq \omega_\tau = 1/\tau$ and $\tau = \eta(r/\beta)$, where η is a numerical parameter whose values range from $\frac{1}{2}$ to 2 and r is the source dimension (Trifunac, 1973)], $F_{NF}(\omega)$ is proportional to $(\sigma\beta/\mu)\omega$.

It should be noted here that the constant high-frequency acceleration spectrum, $F_{NF}(\omega) = \sigma\beta/\mu$ when $\omega \gg \omega_\tau = 1/\tau$, results from a simple ramp in displacement, $d(t) = (\sigma\beta/\mu)t$, at the fault surface (Brune, 1970) and is caused by a Heaviside-type step in velocity at $t = 0$. This is clearly an oversimplification, since the nonlinear behavior of the fault gauge material must limit the accelerations to some finite values and this smooths out the discontinuity of the higher derivatives of $d(t)$ at $t = 0$. This smoothing operation acts as a low-pass filter on $F_{NF}(\omega)$ with the cut-off frequency say, ω_c. As an approximation it might be assumed that this filter is of the form $\omega_c^2/(\omega_c^2 + \omega^2)$.

Unfortunately, there is no experimental evidence that comes from recorded strong-motion accelerograms at this time that could indicate what the realistic values of ω_c are for different types of geological materials at the fault. The closest strong-motion records available, so far, have been obtained at epicentral distances which are of the order of 10 km (Figure 1, 2 and 3) and in many cases the stations were not located on sound igneous rock. Consequently, the low values of Q for the high-frequency waves (say, $f >$ 20 Hz), geometric scattering, and possibly nonlinear response of shallow (less than 200 ft) soils, as well as the current digitization methods (Trifunac *et al.*, 1973), may all interfere with the high-frequency spectral amplitudes in a way that seems to eliminate any possibility of finding out what ω_c might be. On the other hand, several theoretical studies (e.g., Burridge and Willis, 1969; Kostrov, 1964; Richards, 1973) have demonstrated that when the rupture front arrives at a point on the fault the displacement may grow like part of a hyperbola. Such shearing deformations indicate then that the velocities and accelerations can be large in the vicinity of the rupture front; this suggests that ω_c may be quite large for faults that cut through sound igneous rock.

When recorded by a typical strong-motion accelerograph, the spectrum, $F_{NF}(\omega)$, is modified by the acceleration transfer function of the instrument whose amplitude, $H(\omega)$, is given by

$$H(\omega) = \left[\left(1 - \frac{\omega^2}{\omega_n^2}\right)^2 + \left(2\zeta\frac{\omega}{\omega_n}\right)^2\right]^{-\frac{1}{2}} \tag{15}$$

where ζ is the fraction of critical damping. For $\zeta \approx 0.50$, $H(\omega)$ can be approximated by

$$H^*(\omega) = \frac{\omega_0^2}{\omega_0^2 + \omega^2} \tag{16}$$

where $\omega_0 \approx 2\omega_n$.

To compute the expected value of peak acceleration from the Fourier transform given by

$$F_{NF}(\omega) = \frac{\sigma\beta\omega_0^2}{\mu(\omega_0^2 + \omega^2)} \frac{\omega_c^2}{(\omega_c^2 + \omega^2)} \cdot \frac{\omega}{(\omega^2 + \omega_\tau^2)^{\frac{1}{2}}} \tag{17}$$

we employ the method presented by Cartwright and Longuet-Higgins (1956) and the results of Udwadia and Trifunac (1974). According to this method, an estimate of the expected peak acceleration would be given by

$$E(a_{\max}) \approx m_0^{\frac{1}{2}}\sqrt{2}\{[\ln(1-\varepsilon^2)^{\frac{1}{2}}N]^{\frac{1}{2}}+\tfrac{1}{2}\gamma[\ln(1-\varepsilon^2)^{\frac{1}{2}}N]^{-\frac{1}{2}}\} \tag{18}$$

where m_0 is the zeroth moment of the power spectrum, $S_{NF}(\omega)$, of the near-field acceleration

$$m_0 = \int_0^{\infty} S_{NF}(\omega)\, d\omega \tag{19}$$

and $S_{NF}(\omega)$ can be approximated by

$$S_{NF}(\omega) \approx 1/T\,|F_{NF}(\omega)|^2 \tag{20}$$

where T is the duration of strong-motion shaking. In (18) ε represents a measure of power spectrum "width" and is approximately equal to 0.9 for $\zeta \approx 0.5$ and the spectrum $H^*(\omega)$ given by (16) (Udwadia and Trifunac, 1974). N is the total number of peaks of strong-motion acceleration in the time interval T and $\gamma = 0.5772$ is Euler's constant.

For typical strong shaking with duration of 25 sec, $N \approx 250$ and the factor $\{\cdot\}$ in (18) is approximately equal to 2.3 (Udwadia and Trifunac, 1974). The square root of the zeroth moment, $m_0^{\frac{1}{2}}$, then becomes

$$m_0^{\frac{1}{2}} \approx \left\{\frac{1}{T}\frac{\sigma^2\beta^2}{\mu^2}\int_0^{\infty}\frac{\omega_0^4}{(\omega_0^2+\omega^2)^2}\cdot\frac{\omega_c^4}{(\omega_c^2+\omega^2)^2}\cdot\frac{\omega^2\, d\omega}{(\omega^2+\omega_\tau^2)}\right\}^{\frac{1}{2}}$$

$$= \tfrac{1}{2}\left(\frac{\omega_0\pi}{T}\right)^{\frac{1}{2}}\frac{\sigma\beta}{\mu}\cdot R\left(\frac{\omega_\tau}{\omega_0},\frac{\omega_0}{\omega_c}\right). \tag{21}$$

In (21), $R(\omega_\tau/\omega_0, \omega_0/\omega_c)$, which is always less than 1 (Figure 7), shows how nonzero ω_τ and finite ω_c reduce the amplitude of $m_0^{\frac{1}{2}}$. When ω_τ/ω_0 and ω_0/ω_c are small, $R(\omega_\tau/\omega_0, \omega_0/\omega_c)$ is close to 1.0, while for $\omega_\tau/\omega_0 = 0.2$ and $\omega_0/\omega_c = 4$ it reduces to about 0.3. For earthquakes studied in this paper ω_τ/ω_0 is always less than 0.1; if, on the basis of several theoretical solutions mentioned above (e.g., Burridge and Willis, 1969; Kostrov, 1964; Richards, 1973), we assume that ω_0/ω_c should be much less than one, then $R(\omega_\tau/\omega_0, \omega_0/\omega_c)$ is essentially equal to one and may be disregarded in the subsequent calculations which would then apply to reactivation of faults in otherwise sound igneous rock for earthquake magnitudes greater than say $M = 4$.

The natural frequency of a typical accelerograph is about 30 Hz. Typical values of the shear-wave velocity, β, and the rigidity, μ, are 3.0 km/sec and 1.0 to 3.0×10^{11} dynes/cm^2, respectively. For a stress drop (or effective stress) $\sigma = 1$ bar (10^6 dynes/cm^2) and $R(\omega_\tau/\omega_0, \omega_0/\omega_c) = 1$, one gets $m_0^{\frac{1}{2}} \approx 4$ to 10 cm/sec^2/bar; and for $N = 250$ and $\varepsilon = 0.9$, equation (18) then gives

$$E(a_{\max}) \approx 13-33\ \text{cm/sec}^2\text{/bar}. \tag{22}$$

Here we have assumed that $T = 25$ sec and $N = 250$. For $T = 10$, 5 and 2 sec and for the corresponding values of $N = 100$, 50 and 20, the expected value of $a_{\max}$/bar in (22) increases by factors equal to 1.4, 1.9 and 2.5, respectively.

Similar calculations have been carried out by Boore (1973) and Dietrich (1973). Boore, who normalized peak accelerations to effective stress, suggested that the peak acceleration could vary in a linear manner from about 25 cm sec^2/bar, for a high-frequency cut-off equal to 10 Hz, to about 45 cm/sec^2/bar for a cut-off frequency equal to 20 Hz. In his

calculations, a low-pass filter with a cut-off frequency of 10 Hz approximately corresponds to the transfer function of an accelerograph with natural frequency of about 16 Hz and 60 per cent of critical damping. Figure 8 of Dietrich's paper, which presents parametrization of peak acceleration and distance by stress drop and minimum fault dimension for the cut-off frequency of about 10 Hz, suggests a range from about 30 cm/sec^2/bar to about 60 cm/sec^2/bar for peak accelerations at fault. In this paper we employ Brune's theory and the statistics of stationary random functions; Boore in his paper uses non-random approach and Brune's theory, while Dietrich employs deterministic finite element models which can have irregular frictional properties along the fault. In spite of the different methods and assumptions used, these three independent estimates of a_{max}/bar appear to be quite consistent.

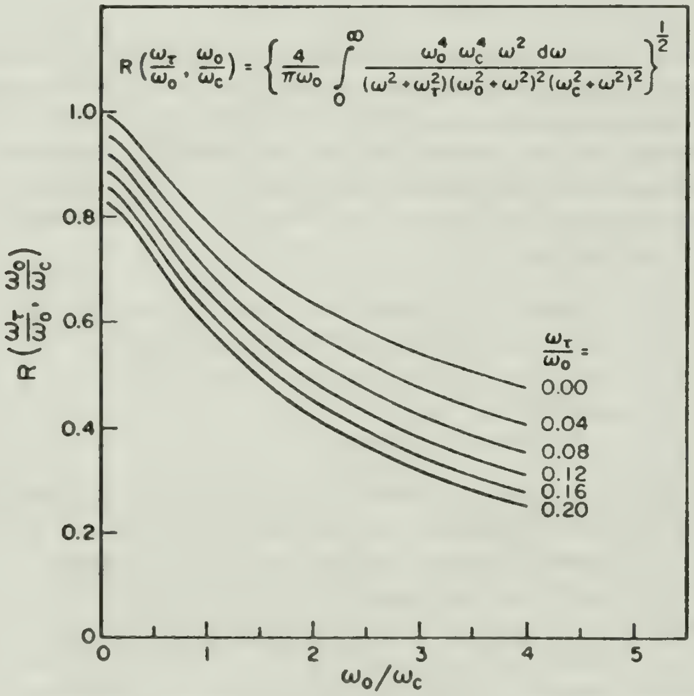


FIG. 7. $R(\omega_\tau/\omega_0, \omega_0/\omega_c)$ versus ω_0/ω_c and ω_τ/ω_0.

In view of the fact that the effective stress may vary from one point on the fault to another, it is not obvious whether a local maximum, local average, or an overall average of effective stress over the entire fault plane should be utilized to compute the expected amplitudes of peak acceleration in equation (22). The local stress maximum does not seem appropriate, since it may be associated with a wavelength too short to be of interest in the frequency band of earthquake engineering (0.05 to 30 Hz). The overall effective stress for a large earthquake could also be eliminated, since it would be associated with the wavelengths too long to contribute significantly to the peak accelerations which are influenced mostly by the amplitudes of intermediate and high-frequency points of the spectrum. We shall assume here, therefore, that the simple estimates of the effective stress which have been collected so far for intermediate and small earthquakes (Figure 6) could be considered as "representative" and in conjunction with equation (22) may be used for independent testing of the amplitudes summarized in Table 7. The perusal of the data in

Figure 6 shows that for $M_L \lesssim 6.5$ only a few of the estimates of stress drop in the Western United States exceed 100 bars. In fact, 90 per cent of all data in Figure 6 fall below about 140 bars, suggesting that the maximum peak accelerations associated with the approximate 90 per cent confidence level might range from about 2 g to about 5 g for the assumed duration of 25 sec. Considering the uncertainties in the foregoing assumptions, which are probably not worse than a factor of about 2 to 3, and the scatter of the computed data, the estimates of the maxima (Table 7) derived on the basis of equation (4) are obviously not contradicted by the estimates based on the simple source theory employed here.

It is interesting to note here that the peak amplitudes of the above maximum velocities and accelerations agree approximately with similar estimates derived by Ida (1973), whose calculations are based on a distinctly different physical basis. The Brune model is governed by the ambient tectonic stress, while Ida's analysis is based on the properties of medium which are related to the material strength against fracture.

Limitations of this Analysis and Possibilities for Future Improvements

Perhaps the most serious limitations of this analysis result from the insufficient number and nonuniformity of available data on peak amplitudes. This is so because most accelerograms come from the recording sites on alluvium (63 per cent) and from a narrow magnitude range between 6 and 7 (71 per cent). Furthermore, over 50 per cent of all data we used come from one earthquake, the San Fernando, California, earthquake of 1971. This may bias our present results, since this earthquake does not necessarily represent a typical shock from the source mechanism or from the instrumental coverage point of view.

Characterization of the amplitude attenuation with distance for peaks of acceleration, velocity, and displacement, which is better than the approximation based on $\log_{10} A_0(R)$, is clearly needed, especially at short distances which are less than about 20 km. In this respect one of the principal limitations of this paper may be related to the fact that we have neglected the dependence of the shape of the $\log_{10} A_0(R)$ curve on the source dimensions for small epicentral distances, where the radiation pattern and the proximity to the fault surface are likely to have a more well-defined effect on recorded amplitudes. When an adequate number of strong-motion data become available for all distances, especially for small epicentral distances, it will be possible to develop better amplitude attenuation laws which will depend on source size and the frequency band and amplitudes associated with the peak amplitudes. Although, as it has been pointed out in the above analysis, the empirical function $\log_{10} A_0(R)$ has several important characteristics which make it suitable for scaling of strong-motion data, it must be pointed out that it only represents our preliminary choice for an approximation of the amplitude attenuation law. Therefore, there must exist other attenuation laws which may describe this attenuation equally well or better than the $\log_{10} A_0(R)$ curve.

Finally, the largest observed peaks of strong ground motion in the near-field will be determined by what can be transmitted through the materials directly beneath and surrounding the recording station. High-frequency high-acceleration pulses which in the linear range would lead to large forces will obviously be reduced by the nonlinear yielding response of the materials which have relatively lower strength.

The accuracy of the estimated dislocation amplitudes $\bar{u}$ in Figure 5, which have been calculated by using equation (11), is within a factor of about 3, which corresponds to 0.5 on the logarithmic scale used in this figure. The accuracy of magnitude determinations is about 0.3 magnitude units for the vertical scale in both Figures 5 and 6, while the estimates of stress drop in Figure 6 could be in error by as much as a factor of 3 to 5. Although

these errors increase the scatter of point estimates in Figures 5 and 6, in the foregoing analysis we assumed that the mean trend of the data is not affected.

We found that the formulas for peak amplitudes, when extrapolated from the available strong-motion data back to the source, at best do agree and at worst are not contradicted by the simple estimates of the corresponding quantities which are based on the source mechanism studies. However, these source mechanism studies have been derived on the basis of a simple source theory (Brune, 1970) which assumes instantaneous stress drop over the entire fault plane and consequently neglects the directional properties of the source (e.g., Ida and Aki, 1972). The Brune theory has been applied often to study the approximate estimates of the parameters which describe the source mechanism, and for many earthquakes the estimates of seismic moment, M_0, and source dimension, r, are now available. With the data from these studies, it now becomes possible to analyze the overall trends and distributions of such estimates and to examine the results from the viewpoint of strong earthquake ground motion. Other more realistic source mechanism models which are not based on the spontaneous stress drop over the whole fault plane require estimation of additional source parameters, e.g., velocity of rupture, and are inherently more complicated to analyze and/or require better data than what is currently available for most earthquakes. Therefore, only a small number of earthquakes in California have been studied by using three-dimensional moving dislocation models (e.g., Trifunac, 1974; Trifunac and Udwadia, 1974), and as yet there are not enough such studies to carry out a comparable statistical analysis as in Figure 5 or Figure 6.

In the formulation of the correlation functions (4) we have omitted source mechanism parameters like seismic moment, stress drop, or radiation pattern. This simplification clearly increases the uncertainty of our estimates, but it could be justified and has been motivated by the fact that in the majority of earthquake engineering characterizations of the potential source of earthquake energy release, typically, only magnitude is used to describe the size of an expected shock. Although source dimension and its orientation in space are often considered in some of the more advanced seismic risk analyses, this information is typically used only to compute the distance to the fault and not to calculate the radiation pattern or possible focusing effects that may result from a propagating rupture.

Conclusions

In this paper an attempt has been made to calculate approximate functional relationships that may exist between the different levels of peak acceleration, velocity, and displacement and such parameters as earthquake magnitude, epicentral distance, site conditions, and component direction. The coefficients employed in these relationships have been presented in a simple form which is convenient for applications. These coefficients represent our preliminary estimates, since the number of available strong-motion records is not adequate to derive the detailed description of the scaling laws involved.

The empirical attenuation curve for peak amplitudes of strong earthquake ground motion in Southern California can be approximated by the $\log_{10} A_0(R)$ curve proposed by Richter (1958) for scaling the peaks of instrumental response, which are used in the calculations of local magnitude, M_L. This can be demonstrated by direct comparison of this curve and the observed peaks for epicentral distances in the range from $R = 20$ to about $R = 200$ km. Although the amplitudes of $\log_{10} A_0(R)$ have been presented for epicentral distances less than 20 km (Richter, 1958), the lack of an adequate number of recorded strong-motion data for this distance range has not permitted evaluation of this

curve for short distances. To circumvent this lack of data, we computed the peak amplitudes for $R = 0$ by extrapolating the results based on epicentral distances greater than 20 km back to the source and assumed that the $\log_{10} A_0(R)$ curve applies there as well. We then compared the resulting peak amplitudes with independent estimates of peak acceleration, velocity, and displacement which could be derived from three studies dealing with source mechanism of earthquakes in Southern California (Trifunac, 1972a, 1972b; Thatcher and Hanks, 1973) and found that there is no apparent contradiction between these two independent calculations.

The approximately parabolic growth of the functions $\log_{10} [a_0(M, p, s, r)]$, $\log_{10} [v_0(M, p, s, r)]$, and $\log_{10} [d_0(M, p, s, r)]$ with magnitude approaches the slope equal to 1 for magnitudes equal to 5 to 6.5. This means that the logarithms of the peak amplitudes of strong ground motion do not grow linearly with magnitude and that this rate of growth becomes very small for magnitudes greater than 6.5 to 7.0. For a magnitude 7.5 shock the peaks of the near-field strong ground motion seem to reach the maximum amplitudes. For magnitude $M = 7.5$, 90 per cent confidence level, and epicentral distance $R = 0$, the estimated maximum amplitudes of strong-motion acceleration, velocity, and displacement are approximately equal to 3 to 5 g, 400 to 700 cm/sec, and 200 to 400 cm, respectively. According to this analysis, these amplitudes would be associated with the largest earthquakes in the Western United States.

The effect of geological site conditions on the confidence levels of peak accelerations has been found to be insignificant. The confidence levels for amplitudes of peak velocities and peak displacements are about 90 per cent and 250 per cent higher on alluvium ($s = 0$) sites when compared with the corresponding level for basement rock sites ($s = 2$).

We found that the amplitudes of strong ground motion in the near-field of earthquake energy release appear to be higher than so far predicted by some previous investigators (e.g., Gutenberg and Richter, 1956; Housner, 1965; Blume, 1965; Milne and Davenport, 1969; Esteva, 1970). These differences can be explained by the serious lack of near-field data ($R < 20$ km) and by the use of somewhat arbitrary methods for extrapolation toward the earthquake source in several previous studies. In this respect, this paper suffers from a similar difficulty associated with the lack of recorded accelerograms for epicentral distances less than about 20 km. However, our use of the $\log_{10} A_0(R)$ function for extrapolating back to the source offers the advantage that the predicted amplitudes of acceleration, velocity, and displacement at $R = 0$ which are based on $\log_{10} A_0(R = 0)$ seem not to be contradicted (within a factor of about 2 to 3) by the amplitudes calculated from other independent source mechanism studies for the representative earthquakes and the same geographic area where most of the strong-motion data which are used in this paper have been recorded. Therefore, while we do expect that our scaling functions and parameters will have to be improved and updated as the data bank becomes more abundant and representative, there seems to be no reason to expect that these changes will be as large as differences between the amplitudes presented in this paper and in several previous studies.

Finally, it should be pointed out here that, from the practical earthquake engineering point of view, high acceleration amplitudes should not necessarily be associated with a proportionally higher destructive potential. An extended duration of strong ground motion and high acceleration amplitudes characterize destructive earthquake shaking, while one or several high-frequency high-acceleration peaks may, in fact, constitute only minor excitation because of the short duration involved and may lead to only moderate or small impulses when applied to a structural system.

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Earthquake Engineering Research Laboratory
California Institute of Technology
Pasadena, California 91125

Manuscript received March 17, 1975

Criticism Perinent to:

Draft Environmental Impact Statement

UCRL-51592

A Geological and Seismological Investigation of the Lawrence Livermore Laboratory Site

L. H. Wight

MS. date: June 3, 1974

Robert S. Zatkin

AUTHOR-TITLE PAGE

-No indication is given of the professional background of the author.

-Is the author a geologist, seismologist or even an Earth Scientist?

-According to the California Division of Mines the author is not a registered California Professional Geologist nor is he a member of the Geologic Society of America.

-Given the enormous risk of plutonium contamination of the environment in the event of a seismic occurance proximal to the lab why was this report not conducted by an independent government agency such as the United States Geological Survey or a team of geologists and seismologists from the University of California.

INTRODUCTION

-(page 1, first paragraph)The investigation was performed under the guidlines of a format put forth (as per reference 1) by the now defunct Atomic Energy Commision.

-Have the guidlines for this type of investigation been revised under the recently fomed Nuclear Regulatory Commision?

-Have discoveries and better knowledge attained in geology and in particular seismology been incorporated into present design standards and analysis formats for instalations such as LLL?

LIVERMORE VALLEY GEOLOGY

Hydrology

-(page 13, first paragraph) The author states a high extraction rate has resulted in a lowered water table underlying the Livermore Valley.

-In the event of plutonium escaping into the environment through a water borne mechanism what could be the consequences of rapid infiltation of plutonium into the ground water table due to a high infiltration rate?

-What of the proximity of the South Bay Aqueduct to the LLL?
-What are the possible consequences on the hydrology of the San Francisco Bay and the people living around it if plutonium were to contaminate this water supply?

GEOLOGIC HISTORY

-(page 14, first paragraph)The author states the occurance of folding and block thrusting on the western boundary of the Livermore Basin by late Pliocene times.

-At no point in the report is mention made of the Verona Fault located along the western boundary of the Basin, No mention of the Las Positas fault trending NE-SW across the Basin.
-Why are these faults not considered in the seismic evaluation?
-Recent NRC determinations of the Verona Fault have placed a Estimated Potential Ground Acceleration along this fault at or in excess of 1.0 gravity, a displacement of 3 meters (confirmed by large-scale trenching). Why are these seismic related values not discussed in the report?

SITE GEOLOGY AND STRUCTURE

Site Geology

-(page 14, first paragraph) The author states the LLL site is on Quaternary alluvium.

-Why is a discussion lacking concerning the soil mechanics of alluviums in general?
-Why is no mention made of analysis of alluvium at the LLL in terms of its particular mechanical behavior? Especially as this relates to strong ground motion and rupturing at the LLL site.

Site Structure

-(page 18, first paragraph on page)The author states the the Tesla fault, of which he deliniates three strands on Fig. 9, is the northernmost segment of the Tesla-Ortiglita fault system. He also states"the northern most extent of the fault is not known but it certainly crosses the Livermore Valley."

-Why does the trace of the Tesal Fault 1, as indicated on Fig. 9. end abruptly at the southeastern border of the LLL site?

-(page 18, first paragraph top of right side of page)The author states the Doutherty Fault is a minor structure which could be of significance only in terms of surface faulting.

-The Doutherty Fault passes within 2000' of the LLL site. Why is no further consideration given to surface rupture on the LLL site from seismic events propagated from this fault?

-Fig. 9 shows a possible branch of the Doutherty Fault occuring NW along the trace of the fault. What of other branches, unknown which might exist to the SW of the faults trace, hence closer to the LLL site?

-The caption to Fig. 10 states cross sections A-A' and B-B' are through the site (one must assume the LLL site).

-As per Fig. 9, cross section A-A' is at least 5000' southeast of the site and as per Fig. ~~10~~ 8 cross section B-B' is adjacent to the southwest border of the site. Why then does the author state in the caption of Fig. 10 that these geologic cross sections are through the site?

Status of Faulting Around the Site

-(page 21, entire page) The author states that movement on the Livermore, Greenville-Riggs Canyon and second strand of the Tesla faults could "reasonably be expected to produce movement on the formerly listed faults" (i.e. the Mocho, Ramp Thrust, Corral Hollow, Doutherty, Carnegie, Patterson Pass faults).

-Why is there no consideration that a large magnitude seismic event on the San Andreas, Calaveras or Hayward faults could initiate movement of any or all of the faults laced throughout the Livermore Valley?

-Table 4 (page 23, first paragraph)

-Geologic evidence (i.e. trenching, bore holes, etc.) for some of the faults listed in this table are not fully explored. In particular the Tesla fault and possible the Corral Hollow Fault, the Doutherty Fault, the Patterson Pass Fault, and the Greenville-Riggs Canyon Fault.

-Why have all known means of deliniating a faults trace and length not been applied to these faults?

SEISMOLOGIC EVALUATION

Bedrock Shaking

Peak Bedrock Accelerations

-(page 25, first paragraph)The author presents the record from the 1952 Kern county earthquake as a representative earthquake for bedrock acceleration determinations at the LLL site.

-Why does the author feel the Taft record "adequately captures the bedrock frequencis" even though the data was not recorded on bedrock?

-The data was recorded at "a range of 42 km" (one assumes by range the author means distance). The Calaveras Fault (EPM=7.5) is located aproximately 10 km from the LLL site. Why is there no consideration of this difference?

-How (mathematically) did the author "scale the accelerations to bedrock values" in the Taft data?

-What type of rock (soil)were the accelerations recorded on in the Taft

record.

-(page 25, second paragraph)The author calculates the soil response by other peoples methods.

-Why is no mathematical treatment (calculations) presented concerning the soil response?

-(page 25, third paragraph)The author refers to boreholes drilled by Shannon and Wilson Inc. and Hersey Oil Company as evidence for bedrock depth.

-No place in the report is the location of these boreholes given. Were they in fact drilled on the LLL site? If so what of log information pertaining to evidence of faulting beneath the site?

-Concerning the borehole of Shannon and Wilson Inc., how was it concluded from a 90' deep borehole that bedrock was located at 400'?

-(page 25, last paragraph) The author presents a bedrock acceleration value of 0.5 g.

-Why are the authors calculations from which the 0.5 g value was derived not presented in the report.

<u>THE SAFE SHUTDOWN EARTHQUAKE</u>

-(page 29, second paragraph) The author states that despite his conlusions and those of reference 2 of the report, the LLL managment "has concluded that the degree of conservatism contained in this report (i.e. the DEIS) and Ref. 2 is excessive" and that a a lower SSE value is to be "recommended for inclusion in all SAR's (Safety Analysis Report) for the site is the SSE in Ref. 3".

-What are the reasons for the LLL managment deciding that the SSE of the DEIS and Ref. 2 are too conservative?

-What is the expertise background of those who made the decision of adherence to the SSE of Ref. 3?

-Why is it that outside seismologists were not consulted in this decision especially given considerations of peak acceleration potentials discussed on page 28 of the DEIS?

APR 12 1979 S.F. Chronicle

Radiation Spill In Berkeley

A cleanup is to continue today at the Lawrence Berkeley Laboratory where late yesterday afternoon a researcher broke a scientific instrument, spilling a radioactive substance.

Laboratory public information officer Charles Hurley said last night that the researcher, Gary Lun, still was being examined for possible radioactive contamination but that there was no hazard to anyone outside the laboratory.

Hurley gave this account of the accident:

At approximately 4:30 p.m. in the lab's Building 50, Lun dropped a clear lucite instrument that held a dust-like compound containing Americium 241, an artifically produced radioactive isotope.

When the instrument struck the floor, the lucite broke, spilling some of the radioactive material.

When Lun left the scene to find a radiation measuring device, he tracked some of the powdery substance into another building and several other employees came into contact with his tracks.

All the other employees were checked for radiation and released after no significant levels were found, but some left their shoes at the lab.

After stripping and showering, Lun was checked for radiation with a whole body counter which found "slight radiation." Tests on Lun continued at midnight. However, those examining him "don't believe there's any danger to him," Hurley said.

The room where the spill occurred has been sealed off except to those involved in the cleanup, which could take several days, he said.

The accident "apparently was a very minor thing," said Hurley, "although after what they did with Three Mile Island and the BART tunnel fire, I hesitate to use the word minor until we're certain."

Inside Livermore's Secret Plutonium Lab

By Marcia Rasmussen

Even though a double layer of protective rubber gloves, solid state plutonium feels warm to the touch, weighs heavy in the hand and emits an odd green dust as it oxidizes.

At the Plutonium Metallurgy Facility at Lawrence Livermore Laboratory strict security prevails, and very few people other than security-cleared nuclear weapons research workers have ever been allowed to get close enough to a hunk of plutonium to "touch" it.

Most of the nearly 7000 lab employees are never allowed within the gray concrete building where plutonium is used in secret federal defense work.

Earlier this week, lab officials invited a small group of reporters inside to look at some of the controversial operations because they will be reviewed in a two-day session of public hearings beginning at 9 a.m. today.

The hearings at Granada High School in Livermore will give the public the opportunity it sought to comment on whether the lab's draft environmental impact statement is an adequate portrayal of the potential risks involved in the operations of the Lawrence lab and its neighbor, the Sandia Livermore Laboratory.

The Lawrence lab, operated by the University of California, does research and produces parts of nuclear weapons for testing for the U.S. Department of Energy. Sandia does research on the non-nuclear aspects of nuclear weapons, a lab spokesman said.

The impact statement is ultimately to be judged by the Environmental Protection Agency when the document assumes its final form sometime this fall.

The environmental review of these and other major federal research labs across the nation was required under an interpretation of the National Environmental Protection Act, even though such statements usually apply only to proposed projects rather than existing ones.

Critics have charged that the lab's assessment is inadequate in several potential problem areas, including earthquake safety, radiation accidents and health effects of doing work with dangerous substances like plutonium in a suburban community.

During the lab tour, reporters peered inside the lab's small research nuclear reactor — a 22-year-old facility now used primarily to help researchers identify substances in soil, marine and other samples for environmental, biomedical and other work.

The group entered the containment area housing the reactor, where the radiation level was measured at five millirems per hour, or about one-fourth the level of a chest X-ray.

At the top of the reactor, a small window permitted a view of the core inside, glowing with the blue light the neutrons make as they speed through the reactor water.

Samples of material whooshed in and out through pneumatic tubes, en route for some of the virtually instant chemistry work performed at the lab. Studies can now be made in seconds of short-lived radioactive isotopes, a guide said.

The highlight of the tour was the facility where a select group of scientists works with plutonium, the stuff used to power some Pacemaker batteries as well as bombs.

The few outsiders who enter here wear plastic booties to avoid tracking plutonium on their feet, respirators to avoid breathing potentially deadly plutonium dust particles if any were to accidentally escape inside the building, and badges. They are surrounded by escorts who answer some, not all, questions about what is going on here, and who make sure that nothing unplanned for is allowed to be seen.

What is planned for is a glimpse of equipment such as a million-dollar remote-controlled lathe purring away on its own, bearing signs that read "Plutonium contaminated."

Visitors were allowed to stick a gloved hand inside the thick rubber sheath of a "glove box" facility to pick up, gingerly a five-pound ingot of plutonium.

The ingot represents the largest amount of the metal that can be worked with in solid form. The limit is an entirely reasonable precaution. It is less than half as much as it takes to form a "critical mass" or chain-reaction pulse of radiation.

"You could hold it in your hand," said Robert Kelley, manager of the plutonium facility. He said the "soft alpha radiation" of plutonium metal "won't even penetrate a piece of paper. It won't penetrate the outer layer of your skin."

But if any plutonium gets inside the body, "you have a problem," he added.

It is the green dust particles that can lodge in a lung and stay there, emitting radiation for the rest of a person's life. The radiation causes damage that leads to cancer, at least in laboratory animals.

Kelley and other lab officials maintained steadfastly that no documented cases have occurred of plutonium causing cancer in hu-

In the face of growing controversy over the suspected dangers in low-level radiation doses, the lab officials said workers at the Los Alamos Scientific Laboratory in New Mexico decades ago received 20 times the normal amount of exposure to the substance and none has died of cancer.

But Kelley and plant services manager James Olsen said strict controls still govern the substances and the workers.

The controls include armed guards, sophisticated detection machines, frequent monitoring of the physical and emotional health of facility staff, air filters, fire safety measures, a special vault for plutonium storage, a stringent accounting system for its use, double checks on everything.

Kelley maintained that the actual dangers are minimal, "as long as we do our job properly."

But human error has led to accidents in the past in lab facilities, including a spontaneous blaze of some plutonium chips left outside a glove box in 1965.

And despite the "analyzed" earthquake resistance of buildings like the plutonium facility, the lab has proposed a series of continued seismic studies to resolve conflicting geologic views of the Livermore Valley.

"The plutonium facility is a very substantial concrete building," Olsen said. But he added, "We really wouldn't like to have a fault under it."

Rad Lab workers tell health fears

Continued from Page 1

He also said the explosives could be set off by lightning, static electricity, human error by technicians or earthquakes. The job, he said, could be done better and more safely with television monitors.

The DOE responded by surveying the site and said the hazards he cited were all within allowable limits.

"That's just about what I expected," said Dias.

But he refused to "babysit" on the firing tables and the laboratory threatened to fire him. He still refuses, but he hasn't been fired yet.

In its report on Dias' complaint, the DOE promised a continuing review of Site 300 employee medical records. Last week, a DOE spokesman could report no definite progress in the review.

But Laborers' International Union, Local 1276, in Tracy which represents 30 of the Site 300 guards and a number of other laboratory personnel, gave The Tribune a partial list, containing 34 names, of former Site 300 workers who have died or become ill in the past 10 years.

According to the union, an extremely high percentage of the deaths has been from various cancers and lung disorders, while a low percentage has been from other causes, such as heart disease. If there were not some cancer-causing agents at Site 300, the union contends, the percentages would be reversed, even allowing for the fact that many of the guards are middle aged.

But the laboratory's medical chief, Dr. Max Biggs, says that is an unwarranted conclusion, that cancer is a very prevalent disease and there are no grounds for such a contention.

The union appears at a double disadvantage:

First, the laboratory keeps all employee medical records under wraps, saying it cannot legally allow access to them.

Secondly, the Occupational Safety and Health Administration has no jurisdiction at the laboratory so all investigations up to now have been in-house.

"They are masters at the cover-up," says Marlin Tolbert, business agent for the union.

The recent discovery by Biggs of an apparently very high incidence of malignant melanoma—14 cases among about 6,300 workers in 12 years—has served to underline Dias' and the union's charges, even though no melanomas were found among Site 300 workers.

The discovery may finally result in an independent review, of sorts, of Site 300 medical records.

At Biggs' request, the Tumor Registry of the state Health Department in Berkeley will take computer tapes containing names and social security numbers of all laboratory employees and correlate them with the registry's extensive computerized cancer records (which all hospitals must report on all cancer patients).

The correlation should enable the state to do a definitive statistical analysis of cancer profiles at LLL—comparing them with Alameda County, the Bay area and the nation.

* Biggs says his own rough calculation indicates the laboratory rate of melanoma is twice as high as that of the Bay area and perhaps four times the national average. But such statistics are a very slippery exercise with many variables.

At the request of The Tribune, Dr. Donald Austin, chief of the tumor registry, said he would identify the Site 300 workers in his analysis, although he will only be able to do so for those living in Alameda County, which will provide a partial analysis of the Site 300 cancer profile.

The review is expected to take several weeks.

A former guard at Site 300 and a friend of Dias is Burris D. Horton, 61, of Tracy.

Horton was stricken with valley fever in his left lung some time ago, then developed cancer in his right lung, which spread to his spine and back. He has been bedridden since last June.

He was a guard for 19 years and before that had a 20-year career in the Marines.

He smoked most of his adult life and concedes that there could be other reasons for his illness, but he says he believes it was his work at Site 300 that finally felled him.

"The hills are covered with beryllium dust," said Horton.

"If the hazard control people

Rad Lab workers tell health fears

Oakland Tribune Feb. 5-78

By NORM HANNON
Science Editor

LIVERMORE—There is something in the air here that has a number of employees at the Lawrence Livermore Laboratory worried—if not alarmed.

Henry Dias, a guard with 18 years' service at the laboratory's Site 300 explosives testing site in the barren hills southeast of here, thinks there is something especially noxious about his post.

He points to a long list of former colleagues who have either died or been stricken with cancer or the fungal "valley fever" lung disease in the past decade.

Dias has been the only one to complain about conditions there, for the record.

"I like working for the laboratory...The pay is good, but I decided they aren't going to mess around with me," said Dias, who is a "reasonably healthy" 53-year-old from nearby Tracy, where a number of the guards live.

Last year Dias filed a formal complaint with the U.S. Department of Energy, for whom the University of California runs the laboratory, where guards have to stand night duty on "classified standby" shots that have been set up to be exploded the following day.

He charged that the firing tables and surrounding hillsides are contaminated with D38 uranium (a "depleted" form of the element used in non-atomic explosions for testing purposes), thorium and beryllium dust, the latter being a potent lung cancer agent.

Continued Page 2, Col. 1

Rad Lab workers ell health fears

tinued from Page 1

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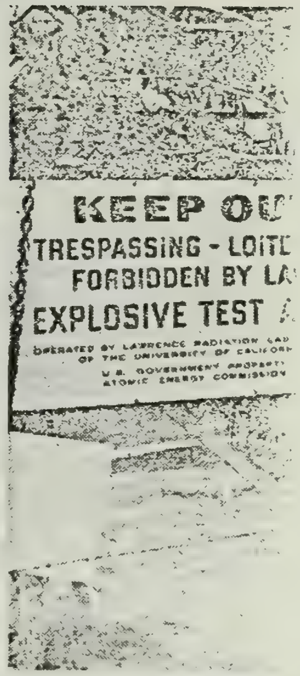


The entrance to Site 300 i

start agreeing with you th there are hazards, they ge transferred," he contended.

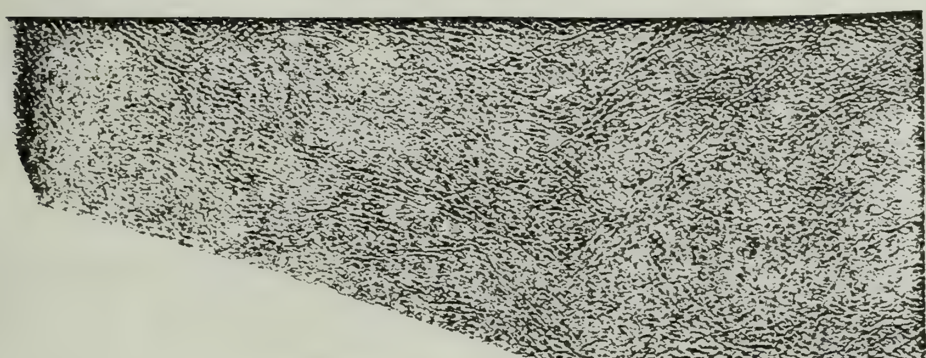
An aerial radiological surve of the laboratory grounds i 1975 (a report which was une plainably left unpublished un October, 1977) showed that ra diatiton levels at Site 300 wer within acceptable limits.

At the laboratory itself, how ever, there were three area where readings fluctuated of the scale.

One strong permanent sourc was found to be radioacti wastes and other materia stored in a large cask in th southeast quadrant of th grounds. Another was the ste in the concrete blocks of the lab oratory's small atomic reac tor. Other sources were transien fluxes of radioactive gases, a gon, oxygen and nitrogen, fr the reactor and the accelerator

All readings, however, wer consistent with the general DO standard that workers can b exposed to no more than 5 rad of radiation per year. The gener al populace can be exposed to n more than half a rad. Bac ground levels are about on tenth of a rad and are absorbe by everyone in the course of lifetime.

One source of radioactivit which the union had complaine about previously, a pallet o drums next to Building 3 which contained oil-soaked de pleted uranium chips from ma chining operations on weapons gave another high reading.



The Cancer 'Cluster' In the Suburbs

May 19, 1978
S.F. Chronicle

By Roger Rapoport

"Our doctor here in Livermore wouldn't believe me when I came back from San Francisco and told him little Jimmy had malignant melanoma," says Billie Long with a faint trace of her native Texas drawl. "He told me little kids just don't get melanoma. He thought the UC Medical Center lab had made a mistake."

Mrs. Long is sitting in the Mediterranean-style living room of her Cinnabar Court ranch home just a few steps away from a handsome family portrait taken shortly before Jimmy's death last November. Nothing in the picture hints at the pain of those final days for Billie, her husband Stan, and their four children.

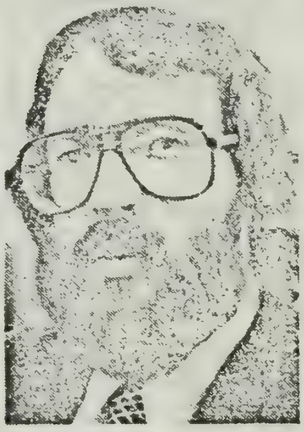
Even Jimmy, who spent the last four months of his life commuting back and forth to Moffitt Hospital where fluid was painfully suctioned from his lungs, looks happy in the photograph. The spitting image of his blonde mother, the seven-year-old sported a healthy tan thanks to endless fishing trips with his father — between visits to the cancer ward.

Stan Long, a mustachioed Pacific Telephone man who brought his family to Livermore's vineyard country ten years ago for the healthy climate, tried during the month following Jimmy's death to think of factors that might have contributed to his son's one in a million case.

Talks with doctors indicated medical experts simply don't know what's behind this cancer which typically appears as a dark mole and spreads from the pigmented skin cells to other portions of the body. The Longs debated over the kitchen table about the X-rays Billie had reluctantly agreed to during her pregnancy with Jimmy. They also considered the potentially harmful effects of food additives, and the hateful smog that frequently blows in from cities to the west.

But it wasn't until late January, when the nearby Lawrence Livermore Laboratory announced that 14 employees had contracted multiple melanoma that they began worrying about the nuclear weapons design facility.

Of course, cancer clusters are not unheard of in various communities throughout the world. And the lab was quick to point out that the odds that 14 out of 6800 employees would develop the same type of cancer were unusual but not significantly high. But this was hardly reassuring to many residents of the town of 42,000. As state health officials moved in to study a cancer incidence that appeared to be more than double the Bay Area rate for [illegible] adults in the



DR. DONALD AUSTIN
He's looking for answers

community began swarming in to local surgeons' offices to have moles removed. Predictably, most of the tissue removed proved nonmalignant.

However within weeks a new wave of anxiety was moving through the Livermore-Amador valley's 150,000 residents. First came a news report on Jimmy's case. Then it was revealed that a 13-year-old Dublin child, whose parents did not want his name made public, was also being treated for the same disease. In Berkeley, Dr. Donald Austin, head of the state health department's tumor registry, voiced concern:

"Two cases in that age bracket for an area of this size is certainly more than you would expect by chance. Over the past five years a Bay Area study of 15 million person-years showed only 7 cases of malignant melanoma under 15. We've never heard of prepubertal clusters like this."

This new development has naturally impelled many Livermore parents to begin poring over their children's bodies searching for moles that might be cause for alarm. A number of these residents have also been directing inquiries to the health department's Dr. Austin.

"People out there are very anxious to have this thing resolved," Austin says. "We've also gotten some tips from technical people out there. One scientist called and told me about a journal article which suggested that rising levels of radioactive Krypton 85 reacting in the presence of the sun's ultraviolet rays could be causing an increase in skin cancer.

"Melanoma of course, is the most serious type of skin cancer. And the Krypton theory is one of a number worth looking into because of a peculiar situation that developed in the Bay Area between 1970 and 1975. "You see, malignant melanoma has been increasing around the nation by one to two percent a year. But in Alameda, Contra Costa, San Mateo and Marin counties the rate has been going up five to ten percent a year. At the same time the incidence in San Francisco has remained unchanged. The only obvious difference between the city and the suburbs is the amount of sunlight. We wonder if San Francisco's fog screens out enough sunlight to have some effect."

While this data hardly reassures those who abandoned the "polluted" city environment for the relative environmental purity of the suburbs, it does not mean one can expect to avoid multiple melanoma by staying indoors with the blinds closed.

Dr. Austin points out that office workers who experience short, intense exposure to sunlight are considered to be running a higher risk than those who regularly work outdoors. That's because regular exposure reduces the risk of sunburn, which may add to the chance of getting the disease.

A few miles away, inside the fenced perimeter of the laboratory that Ernest O. Lawrence and Edward Teller built to help America break into the nuclear age, employees sound philosophical about the problem. At the bio-medical division Dr. Virgie Shore, whose husband Bernard became the lab's second victim of malignant melanoma last January, doubts the suggestion that nuclear materials were a factor. "We never worked with large quantities of radioactive isotopes," says the slim biochemist with short gray hair.

Still surrounded by parts of the vast array of technical literature that they consulted during the two and a half years of her biophysicist husband's illness, Virgie Shore has no qualms about continuing her own work here. "As a scientist you accept the fact that there are certain risks that go with your work. I don't feel in any particular danger."

A team throughout their professional lives, the Shores worked together for the Livermore lab on a variety of projects ranging from blood plasma lipoproteins to the impact of radioactive materials in fallout.

After Shore's cancer was diagnosed in 1975 he and his wife began contacting friends throughout the nation for leads on promising therapies. Among those recommended was a new immunotherapy technique being tried at a San Francis-

co hospital. Unfort… way to qualify wa… enter a "blind" st… up hoping he wou… lucky ones to rece… ly valuable drug. I… worsened Virgie a… cine in her lab, … Bernard was recei… — placebos — inst… thing He dropped … therapy.

From there th… doctors in Walnut … Angeles and oth… of the right ther… tions, chemothera… treatments failed …

The Puzzling Jump in Bay A

By Charles Petit
Science Correspondent

When a close friend at the Lawrence Livermore Laboratory developed malignant melanoma last year, the laboratory's chief physician, Dr. Max Biggs, began to look into the occurrence of the fast-moving, often fatal skin cancer among other employees.

What he found startled him — the rate of melanoma at the sprawling nuclear laboratory appeared to be more than twice that of the general Bay Area population.

His discovery has also made the laboratory's cancer data a part of the broader investigation of why Bay Area rates, for several different types of tumors are going up.

Biggs' close friend at Livermore was Dr. Bernard Shore, 49, who died early last week. His death was followed this week by the disclosure that the State Health Department is actively investigating melanoma incidence at the federal laboratory complex, which is operated by the University of California in southern Alameda county.

The state study resulted from Biggs' request to the California Tumor Registry, operated in Berkeley by the State Health Department, for general statistics with which to compare the 14 diagnosed cases of melanoma among laboratory employees in the past ten years. The laboratory employs about 6800 persons.

So far, detailed statistical analysis has not been performed to confirm that the lab's experience is significantly out of line with what would be expected, or if working conditions such as possible radiation exposure played a role.

But melanoma at one federal facility is not the only thing about Bay Area cancer statistics that worries state health officials.

Dr. Donald Austin, chief of the tumor registry, said that between 1970 and 1975, the overall Bay Area cancer rate rose by about ten percent, including a doubled melanoma rate.

Melanoma, he said, went from about five reported cases per 100,000 persons in 1970 to ten cases per 100,000 persons in 1975.

And other increases, chiefly among women, are equally alarming.

Particular attention is focused on endometrial cancer, which starts in the lining of the uterus.

Stricken Vet Testifi

Washington

Russell Dann, who as a soldier 21 years ago was ordered to watch a nuclear bomb test, sat in his wheelchair yesterday and told a congressional panel that his teeth have fallen out, he is deaf in one ear, has a blood disease and seems to be sterile.

Testifying at a House hearing on radiation hazards, Dann said he suspects the reason for all his ailments is that, as an Army corporal, he had to march within 300 yards of the Operation Smoky atomic test sight in Nevada in 1957 and endure a "sizzling" blast that knocked him off his feet.

Joseph Califano, the secretary of health, education snd welfare, assured the same

RUSSELL DANN AT HEARING
He watched test 21 years ago

In women over the age of 50, the increase is "remarkable," Austin said.

Marin county, he said, "may have more endometrial cancer than any place in the country. Certainly more than anywhere I have heard of."

The Marin incidence rate for this uterine cancer, he said has reached a rate of 341 cases per 100,000 women over the age of 50, more than triple the rate in 1969.

During the same period in five Bay Area counties — Marin, San Francisco, Alameda, Contra Costa, and San Mateo — the combined rate of endometrial cancer increased from 103 to 202 reported cases per 100,000 persons.

For women of all ages, the rate in 1975 stood at 47.5 cases per 100,000, and Austin said "the way things are going, it will overtake breast cancer as the leading cancer among Bay Area women within five years." The present breast cancer rate in these counties is 96 cases per year per 100,000 women.

He said the rising trend here for most tumors is similar to trends nationwide, but the California Tumor Registry data is much firmer than that from other parts of the country.

Women here particularly reflect the rising rate of lung cancer, usually attributed to increased smoking by women and their entrance into jobs with greater exposure to occupational carcinogens in the air. The female Bay Area lung cancer rate stood at 20.6 per 100,000 in 1970, compared to 30.3 in 1975.

The incidence of all cancers rose by 19 percent among Bay Area women, going from 291 cases per 100,000 in 1970 to 346.2 cases in 1975.

The perplexing incidence of melanoma at the Livermore laboratory, while just part of a larger puzzle, illustrates the complexity of finding out what lies behind health statistics.

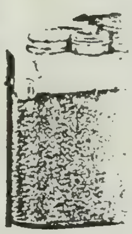
Biggs' first move was to calculate the melanoma rate at the big nuclear weapons laboratory. Based on the average employment over the past ten years, it came to 26 cases annually per 100,000 persons, more than twice the average Bay Area rate of ten per 100,000.

The disparity remained even with a rough correction for the differences in age, race, and sex between the lab population and that of the general Bay Area. The typical laboratory employee — a middle aged white man — has a statistically higher risk for melanoma that the average, Biggs has found.

But with this accounted for, it still appeared the lab should have suffered only about ten cases in ten years, not 14.

Currently, the laboratory is preparing a detailed, computerized summary of employee medical records and backgrounds for Austin's tumor registr[y] precise calculatio[n] number of melan[oma] have been expecte[d]

And, if the ra[te] suspiciously high, working conditio[ns]



ıp in Bay Area Cancer Rate

Vet Testifies About A-Blast

AP Wirephoto

RUSSELL DANN AT HEARING
He watched test 21 years ago

House Commerce subcommittee hearing that the government will expedite its search for other soldiers who participated in the 1957 exercise and later developed leukemia.

Califano said eight leukemia victims have been identified among 500 of the 3153 men known to have been present at the test. But he declined to make a direct connections between the test and the disease.

The panel is probing possible connections between radiation and leukemia.

Dann, 41, of Albert Lea, Minn., was among the Army troops at Camp Desert Rock Nev., on the day of the Operation Smoky blast.

As part of the exercise, hundreds of troops were marched into the immediate area of the blast to conduct maneuvers and determine the efficiency of soldiers in a nuclear blast area.

Dann said his group had been moved from a protective trench to an unprotected hillside vantage point, where they felt the effects of a "sizzling" blast that "knocked me and some other troops ten to 15 feet."

Retired Army Colonel Thomas Stedman of Savannah, Ga., who comamanded a unit at the site, testified that in his judgment safety procedures "appeared to be adequate." But he did not discount the ossibility that individuals could have been affected by the radiation.

United Press

'emale Bay Area lung ·od at 20.6 per 100,000 ıred to 30.8 in 1975.

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the laboratory is ıtalled, computerized employee medical ıackgrounds for Aus-tin's tumor registry to permit more precise calculation of what the number of melanoma cases would have been expected to be.

And, if the rate still remains suspiciously high, investigation of working conditions and possible radiation or other carcinogens could follow.

Shore was among two laboratory workers who died of the disease. He was head of the facility's biomedical division, whose workers often undertake studies with radioactive materials.

However, Biggs said, "I don't think that was a factor. Bernie seldom worked with radionuclides. In fact, radiation exposure doesn't seem to have played a role at all in who got it and who didn't."

Cautious Report on Farallones A-Waste

S.F. Chronicle

MAR 15 1978

By Charles Petit
Science Correspondent

Radioactivity slightly higher than normal — but still non-hazardous — was described yesterday by two groups of investigators who examined the nuclear waste disposal site near the Farallon islands last year.

One report, issued by researchers at the Lawrence Livermore Laboratory, said detailed analysis of fish, worms, and molluscs taken from the ocean bottom and the waters near the old dump site found no radioactivity above levels in similar animals caught elsewhere in the world.

About 220 square miles of the Pacific, south and west of the Farallones and about 50 miles west of San Francisco, were used as a disposal site for low-level atomic wastes, packed into about 47,500 concrete-sealed 55-gallon oil drums, from 1946 to 1965.

The Lawrence laboratory team sampled the area from a boat on the surface in January, 1977, and last October a team from the Environmental Protection Agency used a research submarine to retrieve one of the casks and sample bottom sediments nearby.

The research efforts followed rising public and governmental concern that highly toxic plutonium and other radioactive isotopes could escape from the drums to contaminate fish and other edible marine organisms.

Conclusions from the EPA study won't be available for about six weeks, but the results released yesterday by the Livermore group "sound very much like what we found," said Dr. N. R. Nelson, chief of the EPA's Radiation Source Analysis Branch in Washington.

The isotope known as plutonium 238, while usually less abundant than other isotopes of the same radioactive element, was present at levels ten to 20 times higher than normal in waters and sediments immediately around barrels of the radioactive waste.

The cautiously worded Livermore report said, "This extraneous source deserves additional study."

But Victor E. Noshkin, a nuclear chemist who led the Livermore effort, refused to associate the higher levels with the toxic wastes. "That would be speculation," he said. "I can only describe the measurements."

But while this normally rare isotope of plutonium was elevated, Noshkin said, overall abundance of total plutonium was "exactly what you would expect from global fallout patterns."

Levels of cesium 137, another radioactive element measured in the tests, were also no different from global patterns apparently due to atomic bomb tests by the U.S. and Soviet Union in the 1950s.

And EPA oceanographer Marilyn Barela said the Livermore results are "right in line" with the still-unpublished results of the EPA tests last October.

Those tests, as well as previous surveys of the site, have revealed that up to 25 percent of the nuclear waste drums have collapsed at least partially under the pressure of the ocean 6000 feet below the surface.

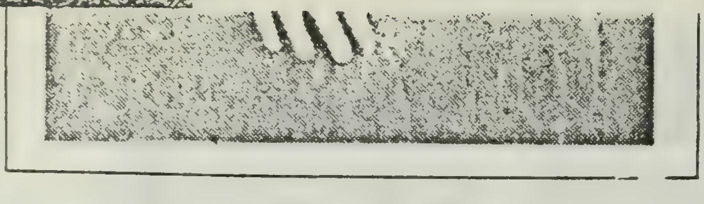
Slightly excess radiation due to plutonium and cesium had been detected, she said, but only within five meters of the casks. "If we had done the tests five years ago, our equipment then would not have been sensitive enough to pick it up," she said.

Noshkin's Livermore group also tested rockfish, bottom worms, and molluscs from the Farallones dump...

The cesium concentration in the rockfish was "unremarkable" and less than half that normally found in some other popular seafoods, including Pacific albacore, Noshkin's team said. Plutonium levels were "well within the levels found in tissues of other marine species collected from midlatitude oceans," according to the Livermore group.

Bottom-living invertebrate worms and molluscs also showed no excess radiation, Noshkin said.

In addition to determining possible hazards of nuclear wastes, Noshkin said, ocean measurement of radioactive particles reveals how pollutants in general are dispersed and carried by currents. Later this year, the Livermore team will undertake similar measurements in the Marshall Islands, near the sites of bomb tests at the Bikini and Eniwetok atolls.



Shore was a biophysicist at Lawrence Livermore Laboratory

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co hospital. Unfortunately, the only way to qualify was by agreeing to enter a "blind" study. Shore signed up hoping he would be one of the lucky ones to receive the potentially valuable drug. But when his case worsened Virgie analyzed the medicine in her lab, and determined Bernard was receiving dummy pills — placebos — instead of the real thing. He dropped out of immunotherapy.

From there the Shores visited doctors in Walnut Creek, Napa, Los Angeles and other cities in search of the right therapy. But operations, chemotherapy, and other treatments failed to slow the disease. The 49-year-old scientist was hospitalized on Christmas Day 1977 and died last January 16.

"The thing that was so terribly hard for him," says Dr. Shore looking up from a diary she has kept of the nightmare, "was to be familiar with the literature and know exactly what was happening. We thought we were doing everything, but in the end the treatment didn't make any difference. Now you sit and wonder if he would still be alive had the doctor been quicker about operating, processing lab tests and putting him on chemotherapy."

But like most other relatives of melanoma victims in the Livermore Valley, Virgie Shore suspects that the only long term answer to the problem is identifying whatever environmental pollutants may be responsible.

For its part, the state health department is now planning to publicize the sudden upsurge of cases in suburban Bay Area counties. They will make urgent recommendations about the value of early detection and reduced exposure to intense sunlight.

Over on Cinnabar Court in Livermore, families such as the Longs are interested in this idea. They and their neighbors have become extremely sensitive to new cancer cases such as a local 7-year-old now being treated at Stanford for another rare tumor called Ewing's sarcoma. Although this disease is not related to melanoma, that fact does little to reassure those who originally moved to Livermore for their health.

"My husband and I are thinking about moving to Sacramento now," says the Longs' neighbor, Anita Redding, who was like a grandmother to Jimmy. "We came here from South San Francisco to get out of the fog, figuring it would be good for our allergies. Our allergies have gotten worse. And frankly all these cancer cases upset us."

Stan Long feels the same way: "I don't know what's causing this cluster out here. It may be the lab and then again it may not. But I think I'd just as soon move to the mountains until they find out what's going on."

Scouting out a new location should be easy for the Longs. Their son had always wanted a camper to use for fishing trips with the family. What with the cost of running back and forth to San Francisco for treatment, such a purchase was out of the question.

But after Jimmy died Billie and Stan took his $2,500 insurance benefit and bought an eight year old camper body that now sits on top of a pickup parked in front of their house. On the back it says "Jimmy's Gift."

"This really was Jimmy's Gift," said his father. "Now we've finally got it for him."

19

BJ Aide
Teacher

in the Travis county juvenile detention center after a hearing before State District Judge Hume Cofer.

George Christian, who was press secretary to President Lyndon Johnson from 1967 to early 1969, was shaken when he and his wife arrived at a police station before the hearing. John is the third of six children in what his father called "a large and happy family."

At the hearing, young Christian looked down at his hands as Municipal Court Judge Alberto Garcia read him his rights. The youth crumpled a tissue and nodded slightly when asked if he understood them. He did not speak.

"There's not any question but he shot the teacher," said Roy Minton, the youth's lawyer.

Because he is only 13 years old, Christian could at most be sentenced to reformatory school until he is 18.

S.F. Chronicle
APR 15 1978

U.S. Probe

Radiation Mishap At Livermore Lab

By Charles Petit
Science Correspondent

A radiation accident affecting three Lawrence Livermore Laboratory employees is under investigation by the U.S. Department of Energy, a laboratory spokesman said yesterday.

Officials said the three men, whose names were not released "for privacy reasons," may have suffered dangerous X-ray exposure to their eyes when a leaded-glass shield was left off a viewing port in a nuclear materials experiment.

Although the three workers have developed no symptoms, such exposure could eventually lead to cataracts and possible blindness . . . years or even decades from now. Exposure to high-dose X-rays also carries an increased risk of eventual cancer.

The three workers were running a test earlier this month in which samples of carbon, tantalum and tungsten were bombarded with a beam of high-energy electrons, a process that produces intense X-rays.

The test apparatus is in a temporary research trailer that is part of the laboratory's isotope separation facility. It is shielded by lead, but has a one-and-one-half-inch viewing port that is supposed to be covered by transparent, leaded glass.

The exposure of the workers was discovered Tuesday when their dosimeters — devices pinned to the clothing of laboratory employees to determine their accumulated radiation exposure — were checked by safety personnel.

The results showed alarmingly high levels of exposure, leading to the discovery that the protective shield was not installed on the viewing port during at least some of the experiments between April 3 and 10. A team of investigators from the Department of Energy is now leading an effort to duplicate the error in order to determine precisely how much radiation exposure the men received.

Some experiments at the laboratory involving hazardous materials or radiation are equipped with interlocks to prevent tests from being run unless all safety procedures are followed.

However, no such precaution was taken before the accident involving the three workers, according to a Livermore spokesman. "Machines like the bevatron at Berkeley shut down automatically when someone opens the wrong door or something," he said, "but I don't think we do this for portholes and the like." (The bevatron is the high-energy atom smasher at the Lawrence Berkeley Laboratory.)

No accurate estimate of the exposure could be gotten from the dosimeters pinned to the workers' shirt pockets, he added. "They cannot be correlated with the exposure to the eyes," he said. "They just indicated something abnormal had occurred."

The investigation is expected to take several weeks and the exposed employees are still at work at the laboratory.

A team of five investigators from the energy department's regional office in Oakland is leading the probe.

If the inquiry confirms that the radiation exposure was in fact dangerous, it would be the fourth such incident reported in the history of the sprawling facility, operated for the federal government since the early 1950s by the University of California.

S.F. Chronicle FEB 1 5 1978

Report Criticized on UC A-Weapons Lab

A special committee's report that the University of California should continue to manage two nuclear weapons research laboratories was called a "classic administrative whitewash" yesterday by opponents of the university's role in the research.

At a press conference on the Berkeley campus, members of the Nuclear Weapons Labs Conversion Project issued their own recommendations for the Livermore and Los Alamos labs as an alternative to findings made by the special committee last week.

That eight-member panel, headed by former UCLA vice chancellor William Gerberding, urged continued operations of the labs by UC but called for creation of a special nine-member board of overseers to survey laboratory programs and keep them from getting "isolated from the larger world of thought and action."

Speaking for the anti-nuclear project, UC physics professor Charles Schwartz said the makeup of the Gerberding committee was lopsided with the "uppermost hierarchy of the university."

Its report, he charged, was "so lacking in commitment, clarity and plain common sense that it is unlikely to be of any use to the university or the general public."

In its alternative proposals, the project urged the regents to convert the labs to "basic energy research that would meet our increasing need for safe, renewable energy sources."

Study of Cancer Near Arms Plant

S.F. Chronicle APR 11 1979

Denver

People who live downwind from the Rocky Flats nuclear weapons plant near Denver have substantially higher cancer rates than Denver-area residents not exposed to the plutonium emissions, according to a report made public Monday by the director of the health department of Jefferson county, where Rocky Flats is situated.

The study showed that men living up to 13 miles downwind, or generally east, of the plant had a testicular cancer rate 140 percent higher than would be expected on the basis of cancer statistics for the Denver-area residents. Throat and liver cancer rates were 60 percent higher than expected, and rates of leukemia and lung and colon cancer were 40 percent higher.

The study showed that in the area up to 13 miles downwind from the plant, overall cancer rates were 24 percent higher than expected in men and 10 percent higher in women. In the area 18 to 24 miles downwind from the plant, cancer rates were 8 percent higher in men and 4 percent higher in women.

The study was conducted by Dr. Carl J. Johnson, a physician and epidemiologist who, in addition to being county health director, is associate clinical professor of preventive medicine at the University of Colorado Medical School.

Johnson based his study on cancer incidence data for 1969 to 1971 collected by the Colorado Regional Cancer Center for the National Cancer Institute. The data reflected cancer rates among 595,226 whites who live up to 24 miles downwind from Rocky Flats and for 423,866 whites in the Denver area not exposed to the plant's plutonium emissions. The study controlled for variables.

The Rocky Flats plant, which covers ten square miles, is the nation's only facility producing the plutonium cores used in nuclear weapons. It is 16 miles northwest of Denver, and is operated for the federal government by the Rockwell International Corp.

The study was first made available by a representative of the University of California Nuclear Weapons Lab Conversion Project. The Berkeley-based organization is opposed to nuclear weapons development at the Lawrence Livermore Laboratory, a facility operated for the Department of Energy by the University of California.

Johnson said he would discuss his study in Livermore tomorrow at a hearing on the environmental impact of the Livermore lab, some of whose weapons use components produced at Rocky Flats.

Johnson's findings contradict a 1977 environmental impact statement, prepared by the Energy Research and Development Administration, that said radiation from plutonium emissions at Rocky Flats had the potential to cause one cancer death and one genetic defect among the 1.6 million people living in the Denver area. The Johnson study indicates 501 unexpected cancer cases downwind from Rocky Flats.

In addition to what the study called "routine plutonium emissions that began in 1953," the Rocky Flats plant released high concentrations of plutonium into the atmosphere in 1957, 1968, 1969 and 1974. In 1957, a fire at the plant resulted in a release of plutonium in amounts 19,000 times the present maximum set by the Department of Energy, which took over the functions of the ERDA in 1977, the report said.

Johnson said the higher incidence of testicular cancer in plutonium-exposed men was "particularly significant" because scientists at Rocky Flats have shown in animal studies that ingested plutonium tends to collect in the testes.

New York Times

To: Mr. G.V. Beard
Mr. J.B. Farmakides
Mr. L.T. Grose

The recommendations made by Tony Barreiro from S.W.O.N. (Santa Cruz) for the final EIS on Livermore Labs & the Sandia Labs:

1. Assess the cancer rate among lab workers & Livermore residents.
2. ~~[illegible]~~ Include in the report a statement that many medical experts question the actual safety of federally established maximum permissable doses of radiation + radioactive materials.

3. Assess the longterm waste disposal problems arising from LL operations.
4. Assess the environmental impact of the arms race.

5. Give full open-minded treatment to the proposed alternative of peace conversion of the LL labs.
6. In the short-term, support removal of all radioactive materials from Livermore to the Nevada test site.

[signature]
Anthony L. Barreiro
Annette C. Dow
Annette C. Dow
[signature]
Ann-Marie C Hendrickson

April 4, 1979

Mr. W. H. Pennington, Deputy Director, Office of Environmental Compliance
Department of Energy
Mail Station E-201
Washington, D. C. 20545

Dear Mr. Pennington:

I request the opportunity to speak at the public hearing on April 12th, at the Granada High School in Livermore, California. I believe that my statement will not take in excess of ten minutes; probably less.

It will include the following:

First: my observation that the Draft Environmental Impact Statement seems to have been drawn up by the Staff of the Lawrence Livermore Laboratory itself. And that, if this be the case, the result is not an impartial study and report. For whereas the mathematical data therein can be, and should be checked for accuracy, the interpretation of these data would be highly subjective; as would be any decisions for needed changes that might be indicated by them.

Second: I note, in certain areas, quite general, rather than specific statements. For example, P 1-1, 4th paragraph:" ---today ...programs include magnetic fusion research, peaceful uses of nuclear explosives, bio-medical studies, laser fusion and laser isotope separation research." And, in paragraph 5, "Anticipated national benefits ...include energy programs in geothermal...coal...and solar energy." We need to know, percentage-wise, how much research, in process and planned, in comparison with that for nuclear weapons research. In this paragraph also is clearly shown the non-objective thinking of those who wrote this report: "Benefits to national defense have resulted from the nuclear weapons development program." This is an opinion, not a fact. However widely held, it is now increasingly challenged by a growing number of citizens, including eminent scientists.

Third: Concerning Health: Where the background radiation is referred to as a norm, in assessing the comparative doses received at the site or near it, no account is made of the fact that background radiation itself, in the Livermore area, has been increasing over the twenty-five years of the Laboratory's experiments. In other words, background radiation, for comparative purposes, should be taken from an area where nuclear research has not been carried on; particularly from an area not as close as the whole Bay Area is to the Nevada test site.

Very sincerely yours,

Grace Dilley

Grace Dilley

1399 Queens Road
Berkeley, California
94708
(415) 845-3259

ENVIRONMENTAL COALITION ON NUCLEAR POWER

[redacted] [redacted]

Executive Directors: George Boomsma—R.D. #1, Peach Bottom, Pa. 17563 717-548-2836 / Judith Johnsrud—433 Orlando Avenue, State College, Pa. 16801 814-237-3900

119 E. Aaron Dr.
State College
Pa., 16801
9 April 1979

Mr. W.H. Pennington
Depudy Director,
Office of Environmental Compliance
and Overview
U.S. Department of Energy
Mail Station E-201
Washington, D.C.
20545

Dear Mr. Pennington:

Enclosed is my response to the DOE Staff responses to comments received on the DEIS for the Livermore Site, March 1979. I would request that these be included in the record of the hearing to be held on this matter. I will be unable to be present at the hearing in Livermore, California on 12 April 1979, due to commitments here. I would further request that the hearing record be held open so that I may respond to the Staff concerning my remarks. I seek only to clarify the issues presented in my enclosed response and in my original comments. I expect this can be done quickly. I apologise for the delay in this submission. The events at Three Mile Island Unit 2 required considerable time and effort.

Sincerely,

William A. Lochstet

Wm. A. Lochstet

Response to DOE Staff Statement on
Comments Received on
DEIS for Livermore Site, March 1979

by

William A. Lochstet
Environmental Coalition on Nuclear Power
April 1979

The U.S. Department of Energy Staff has prepared a Statement in response to comments redeived on the Draft EIS for the Livermore Site (DOE/EIS-0028-D), which was issued in March 1979. Three specific responses from the staff under the waste management section need to be addressed.

The second comment and response under waste management (page 19 - middle of page) concerns ,expressed by commentors, onsite radioactive waste storage at LLL and ultimate shipment offsite. The DEIS does not address the environmental impacts of such operations. The ultimate disposal of this waste is dismissed by the staff in its last sentence which says that once these materials leave the Livermore site, the responsibility for safety is transferred to the waste management facility. We shall not be concerndd with transportation for the present. If the waste is produced by LLL operations, then the benefits of that activity accrue to LLL operations, so must the environmental impacts of its disposal. This is true even if the "responsibility" for such disposal lies outside LLL. If LLL did not exist, the wastes would not need to be disposed of. This undeniable connection was recognized by the court in footnote 12 of NRDC v. USNRC, 547 F. 2nd 633 (D.C. Cir. 1976), which states in part:

> We note at the outset that this standard is misleading because the toxic life of the wastes under discussion far exceeds the life of the plant being licensed. The environmental effects to be considered are those flowing from reprocessing and passive storage for the full detoxification period.

This statement clearly links the operation of the plant to the totality of impacts resulting from waste disposal. The waste disposal was to be carried out by the U.S. Government, which was not the owner of the plant. Even though the responsibility for waste disposal is transfered to another entity, the environmental impact is not.

The third comment and response in the waste management section (Page 19 bottom) concerns burials of waste at Site 300, and the need for an environmental impact analysis. The Staff response seems to imply that if the requirements of 10 CFR 20 are met, there is no environmental impact. If these requirements are met, the environmental impact is deemed acceptable. The environmental impact should be evaluated by the Site 300 monitoring program, as stated by the Staff. This evaluation should be included in the FEIS. This environmental impact should also be evaluated by studies which attempt to predict future movements of this material. These evaluations should be included in the FEIS, also. This enclosure is required by footnote 12 of NRDC v. USNRC as quoted above.

The comment concerning the health consequences of radon - 222 emanating from the depleted uranium waste at Site 300 is adressed at the bottom of page 21 and the top of page 22. The Staff points out that this depleted uranium is essentially free of

radium - 226, the parent of radon - 222. This is essentially correct. This is the present composition of the waste, but it does not remain so. The depleted uranium is largely uranium-238, which decays radioactively by alpha emission to thorium-234. Thorium-234 in turn decays thru protactinium 234 to uranium 234, by beta decays. Uranium-234 decays thru thorium-230 and radium-226 to radon-222 by alpha decays. With the passage of time, this process will lead to an increasing radium-226 content, and an increasing radon-222 generation rate. This process operates on a time scale determined by the 4.5×10^9 year half life of uranium-238. The original comment concerning radon-222 emissions should be fully considered in this light. The FEIS should include the impacts of these radon emissions.

1008 Willow Drive
Lafayette, CA 94549

April 14, 1979

Dr. John Farmakides, Chairman
DEIS Advisory Board
U. S. Department of Energy
Washington D. C. 20545

Dear Dr. Farmakides:

Why is it assumed that development of nuclear weapons (the stated purpose of the Lawrence Livermore Laboratory) is in our national interest? Professor Ellsberg showed that the new weapons developed by the Lab are used to back up first use threats, while the American public has been hoaxed into believing such weapons are needed as deterrents. Surely this is the single most important question for your committee to consider. The ultimate environmental impact is nuclear holocaust. What other eventuality can you foresee - unless we stop searching for ever more lethal weapons?

Another totally unexamined assumption which is made throughout the DEIS is that there is ANY safe level of radioactive pollution (emissions or effluents "within government guidelines"). That is tantamount to saying "for this purpose it is okay to kill a certain number of people and engender a certain amount of genetic damage". Independent scientists have no doubt that naturally occuring background radiation accounts for some percentage of the cancers and genetic damage our population now experiences. Adding human-made radioactive pollution to the environment inevitably increases the number of deaths from cancer and leukemia and the number of harmful mutations proportionately. Worst of all, it is not just our generations which will pay (are paying) these frightful costs. Contamination of the earth with radioactive particles of long half-life, some of it concentrated ominously through the food chain, is our deadly legacy to any who may live in the coming millenia.

I appreciated your patience and fairness during the hearing which I attended from nine to five o'clock. I am a concerned citizen, nearing 65. I should like future generations to be able to love the gift of life as I have.

Sincerely yours,

Dorothy Headley

Dorothy Headley

UC Nuclear Weapons Labs Conversion Project

War Resisters League West
1360 Howard Street
San Francisco, CA 94103
(415) 626-6976

Berkeley Students for Peace
608 Eshleman Hall, UC Berkeley
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Ecumenical Peace Institute
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2740 Hillegass Avenue
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April 16, 1979

Questions from Region
to staff

Mr. Richard A. Du Val
Asst. Manager for Programs
U.S. Department of Energy
1333 Broadway
Oakland, CA 94612

Dear Mr. Du Val:

Thank you for agreeing to answer the questions raised in the Conversion Project's December 18, 1978 critique of the Livermore Site Draft Environmental Impact Statement which were not addressed in the Energy Department's staff response in March, 1979.

In thé interest of obtaining thoughtful responses from you, I have actively self-censored, omitting not only those questions directly addressed in the staff statement but also those whose answers I can infer from the statement and from your answers to questions at the hearing last Thursday. There are two exceptions. One, #7D, is modified to be self-explanatory and, I believe appropriate. The other, #11A, was not dealt with in the staff response on Mission of the Laboratories.

At the end I have included five questions raised but not answered in the hearings. All of them are directly related to concerns addressed in our December critique and so do not represent an attempt to raise new issues. All are relevant according to DOE criteria. All are unquestionably important.

<u>1B.</u> Maximum credible spill makes the wholly unwarranted assumption that the building air filters continue to work fully (3-65). Serious accidents occur when there is a multiple failure of several safety precautions. A proper risk evaluation will consider such cases; why didn't yours?

<u>1C.</u> Why was whole soil sampling rather than respirable dust sampling used to determine plutonium hazards both on-site (2-62 to 2-65) and off-site (Appendix 3C)? There was a response to this question in the staff statement (page 6), but Dr. Johnson, Ronald Tidball and Ronald Severson answered those criticisms of his method in a letter published in <u>Science</u> two years ago (enclosed). Since the subsurface contamination of soil by plutonium is also of interest, both methods should be used; the incompleteness of his method is hardly reason to reject it, particularly since, of the two, his is the more inclusive in terms of risk.

1D. There are no Federal guidelines governing exposure to Americium (in all forms), Californium (in all forms), Thorium 230 and Radon 222. Why is there no discussion of how the Lab governs the use of these elements?

3A. Will there be in the next draft of the EIS summaries for each year of exposures by Lab personnel to radioactive substances?

3B. In the analysis of the ARMS flyover (2-66) it is asserted that of the 11 radioactive spots targeted by the flyover, "None of these represent a radiation hazard to workers in the area." Where is the evidence to support that assertion?

In the analysis of dispersal of radionuclides by testing activities at Site 300, the claim is made (2-66) that debris scattering is confined to a radius of 500 yards and thus "... the areas affected are well within Site boundaries and the uranium levels observed constitute no health hazard." Why is no evidence offered to show that the debris is in fact confined to a 500 yard radius of the firing tables? Does this include very small particulates? Why is no evidence offered to support the assertion that the uranium (and beryllium) debris constitute no hazard to Site 300 workers?

3C. In the section entitled "Accident Analysis" (3-47 et seq. and Appendix 3C), there is neither acknowledgement nor analysis of the dangers to Lab personnel of accidents having "off-site significance." Why not?

3H. Has consideration been given to suspending operations in Buildings 251 and 332, noted by the DEIS as requiring upgrading of their effluent control systems, until such time as the improvments are in place?

4D. Will there be a listing in the next draft of all radionuclides currently on the site (according to the staff statement, toxic substances will be listed)? Will average annual amounts of both kinds of substances be included?

4E. What are the total amounts of radionuclides unaccounted for? What amounts are unaccounted for in 1977?

4F. Will the next draft include a summary of the releases of radionuclides for each of the Lab's years of operation (cf. DEIS table 3-1, page 3-22)?

4J. Has there been disclosure to officials in all counties through or over which the Lab ships radionuclides? Do each of these counties have emergency plans for dealing with an accident involving one of these shipments in the event containment vessels are breached?

5C. If sewer effluent becomes contaminated and is redirected to storage, how many minutes of storage are available before the contaminant must be dumped in the Niles Canyon sewer? If the contaminant is very dangerous, how long can the effluent be stopped before cooling water supplies would be cut off? With the sewer diverted to holding, is the working population evacuated from the Site?

5D. The Lab's sewer effluent monitoring station is in the immediate proximity of the Tesla II Fault. What protections

does the station have against disruption by an earthquake? What additional protections do the back-up systems have?

5E. How long has it taken to detect and to report to state authorities (and to relevant county authorities) each contamination incident that has had off-site significance?

7A. The DEIS notes at 3-20 that radioactive and toxic liquid wastes are poured into the Livermore municipal sewer system through a process of controlled dilution either at the site or, if that fails, by gradual release from segregation tanks in the municipal system. What are the containment limits for effluents put into the sewer system (3-19 and 20)?

7B. Reference is made on the same page (3-20) to options in the event of a large release into the municipal sewer system. What are those options?

7D. Is it true that neither the Nevada Test Site Impact Statement nor the Livermore Impact Statement has an environmental impact analysis of the nuclear weapons testing programs? If so, where is the analysis of the impact of that program to be presented?

9. Are there plans to undertake transmutation of heavy radionuclides at the Lab? If so, will this be discussed in the next draft?

10. Under the discussion of "other Lab activities", will there be a discussion in the next draft, in the section describing the benefits of laser isotope separation and laser fusion technologies, mention of the centrality of tritium as a plasma (a negative impact) and the production of plutonium as an intermediate step (another negative impact)? Will there be discussion of the weapons applications of laser fusion?

11. I understand that you do not believe a discussion of the merits of further nuclear weapons development and of the impacts of such a policy to be relevant to the Impact Statement. However, in your response you did not address either of the points we raised. Would you restate your response to include them? They are reiterated below:

1. It has been held by the Federal courts in a successful suit against then-Secretary of the Interior, Rogers Morton, that, under the National Environmental Policy Act, the absence within an agency of the ability to implement a course of action does not excuse it from an obligation to explore the wisdom of the course of action.

2. The Department of Energy has in the past formulated its own position on nuclear policy which was at odds with the Administration's. For example, Secretary Schlesinger and the Lab Directors (Agnew and Batzel) have openly opposed the proposed five-year Comprehensive Test Ban Treaty.

I want to add a third point for your consideration, if I may.

3. Presumably, the development of unspecifiable new weapons through research and development is not covered by the Stockpile Paper, alluded to in your response.

13. Inadequate consideration of alternative plans for the Lab that would minimize or eliminate existing hazards.

The National Environmental Policy Act requires the following of each Impact Statement:

> A rigorous exploration and factual evaluation of the environmental impacts of the full range of reasonable alternatives to the proposed action shall be presented. (10 CFR 1021.41 (c)(8))

The present draft devotes three and one half pages to the consideration of four alternatives. Will they be elaborated in the next draft?

14F. What agency if any actually checks the Livermore Site to determine if the Lab has overlooked the danger of some activity beyond what is reported in the DEIS?

That concludes the list of the most important unanswered questions from our December critique. Below are the five additional questions alluded to in the beginning of this letter.

1. Were there any accidents at the Site before 1960 that had off-site significance or the potential for off-site significance?

2. Would you list for me all burial sites for radioactive waste ever used by the Lab within 100 miles of the Lab?

I know that you responded to the best of your knowledge to both of these questions at the hearing. They are of such importance, however, that definitive answers are called for.

3. Will you be responding soon to the questions and requests for information submitted by Dr. Carl Johnson?

4. What is your position on his call for an immediate study of cancer incidence rates and rates of congenital malformation among the populace living within twenty miles of the Lab?

5. What is your position on the request by Dr. Kenneth Miller for an analysis of the cancer incidence rates among higher risk subpopulations of the Lab's people?

If I can be of help in this admittedly tedious process, please call.

Sincerely,

Will Riggan

Will Riggan
for the UC Weapons Labs Conversion Project

cc: John B. Farmakides
L. Trowbridge Grose
G. Victor Beard

References and Notes

1. I. Gormezano, N. Schneiderman, E. Deaux, I. Fuentes, *Science* **138**, 32 (1962); N. Schneiderman, I. Fuentes, I. Gormezano, *ibid.* **136**, 650 (1962).
2. T. W. Berger, B. E. Alger, R. F. Thompson, *Science* **192**, 483 (1976); C. Cegavske, R. F. Thompson, M. M. Patterson, in preparation; T. W. Berger and R. F. Thompson, *Science*, in press.
3. For example, J. H. O'Brien and S. C. Packham, *Cond. Reflex* **8**, 116 (1973); J. H. O'Brien and S. S. Fox, *J. Neurophysiol.* **32**, 285 (1969); U. G. Gasmov, *Neurosci. Behav. Physiol.* **6**, 189 (1973); N. M. Weinberger, T. D. Oleson, D. Haste, *Behav. Biol.* **9**, 307 (1973); D. D. Wickens, P. M. Meyer, S. N. Sullivan, *J. Comp. Physiol. Psychol.* **54**, 572 (1961).
4. J. Olah and M. M. Patterson, in preparation.
5. For example, S. R. Coleman and I. Gormezano, *J. Comp. Physiol. Psychol.* **77**, 447 (1971); M. C. Smith, *ibid.* **66**, 679 (1968).
6. C. F. Cegavske *et al.*, *ibid.* **90**, 411 (1976); A. Rosenbluth and P. Bard, *Am. J. Physiol.* **100**, 573 (1932).

7. Supported in part by National Institute of Neurological and Communicative Diseases and Stroke grant 1-R01-NS10647 and American Osteopathic Association Research Bureau grants. We thank L. Towns for his helpful comments on the manuscript.

28 December 1976

Measuring Plutonium Concentrations in Respirable Dust

In their report on the plutonium hazard in respirable dust, Johnson *et al.* (*1*) state that "the respirable fraction of surface dust was separated by ultrasonic dispersion and a standard water-sedimentation procedure." It is apparent that their respirable fraction includes particulate that is too large to fall within the respirable size range, and that the analytical results obtained after the sample preparation techniques described will not show the concentration of plutonium associated with the in situ respirable surface dust. My criticism has as its basis the following reasons.

1) Wet digestion with hydrogen peroxide and particle dispersion by sonication reduces or eliminates the binding mechanisms that hold respirable-size plutonium particles to nonrespirable-size dust particles in the surface soil. After altering the real in situ particle associations, it is wrong to assign the final value for the soil concentration of plutonium to the original respirable size fraction of the surface soil.

2) In using the sedimentation technique to isolate the respirable size fraction, it is wrong to base "threshold parameters" on particles having an effective maximum diameter of 5 μm and a density of 11.45 g/cm^3 because (i) A 5-μm PuO_2 particle having a density of 11.45 g/cm^3 has an equivalent aerodynamic size of about 17 μm, which is well above the respirable size range. (ii) By using the above threshold parameters, one includes in the respirable fraction much of the ordinary dust present that is well beyond the respirable size. For example, by Stokes' law, dust particles with a density of 1.5 g/cm^3 and a size of 23 μm may be shown to have sedimentation characteristics similar to those of PuO_2 with a size of 5 μm and a density (ρ) of 11.45 g/cm^3. A 23-μm (ρ = 1.5 g/cm^3) dust particle has an aerodynamic diameter of about 28 μm. It is the aerodynamic diameter that determines respirability.

This selection of threshold parameters may or may not give conservative results in assessing the hazard of plutonium in soil. The plutonium attached to host dust particles that are well above the respirable size range is included as respirable particulate, while nonrespirable dust particles with no attached plutonium are also included in the respirable dust fraction.

In any event, these methods of sample preparation and data analysis will not yield valid results. The most conservative approach would be to call all of the plutonium respirable, since that which is outdoors is virtually all in the respirable size range (the mean size at Rocky Flats is on the order of 0.3 μm or less, depending on source) when considered as unassociated with host soil particles. If one wants to know the concentration of plutonium actually associated with respirable dust particles, then a valid technique must be used. One such technique would be to sample by vacuum and collect by impaction, using an impactor that classifies the dust according to its aerodynamic size.

JOHN A. HAYDEN

Rockwell International, Rocky Flats Plant, Golden, Colorado 80401

References

1. C. J. Johnson, R. R. Tidball, R. C. Severson, *Science* **193**, 488 (1976).

1 November 1976

We believe that it is valid to disperse the in situ particle associations. The procedure is used in an effort to overcome the variables associated with microaggregate stability, in order to achieve reproducible results and provide data that are comparable from season to season or site to site. Our definition of the respirable size fraction is that fraction of soil that includes plutonium oxide particles of the given size. This fraction does include other mineral particles of lower density and larger diameter (to 12.6 μm, based on an average mineral particle density of 2.65 g/cm^3, according to Stokes' equation).

It is irrelevant whether these other mineral particles are ever retained within the lung, although there is evidence of some retention (*1*). However, that does not render it unacceptable to use the weight of the entire fraction as a basis for expressing the concentration of the plutonium. This fraction does comprise the orders of particle sizes of concern for health.

We agree that the selection of threshold parameters could be based on an appropriate equivalent aerodynamic size in place of the actual particle size. However, this is not necessarily a more conservative approach for the conditions of this study. It is probably true that nearly all of the plutonium on the soil is in the respirable size range (*2*), and we have probably measured nearly all of the plutonium on the surface of the soil. A minor adjustment in threshold parameters as proposed would result in a small change in the weight basis for expressing concentration. We believe that this concentration difference is trivial, particularly when compared with the difference between the weight of the respirable fraction (following our definition), which we used, and the weight of the whole soil collected to arbitrary depths, which it has been the practice to use in the past.

Employing a vacuum device for sample collection may be a useful modification of our method, if the device is equipped to avoid loss of submicrometer particles. The respirable fraction may be separated by any procedure capable of performing the separation effectively. However, the separation procedure that we utilized to isolate the respirable fraction will probably yield more reproducible results (*3*).

CARL J. JOHNSON

Jefferson County Health Department, Lakewood, Colorado 80226

RONALD R. TIDBALL

RONALD C. SEVERSON

U.S. Geological Survey, Federal Center, Lakewood, Colorado 80225

References

1. J. D. Brain and P. A. Valberg, *Arch. Environ. Health* **28**, 1 (1974).
2. J. C. Elder, M. Gonzales, H. J. Ettinger, *Health Phys.* **27**, 45 (1974).
3. G. W. Kunze, in *Methods of Soil Analysis*, E. A. Black, Ed. (American Society of Agronomy, Madison, Wis., 1965), part 1, pp. 568–577.

23 March 1977

Tab 3

Transcript of the Hearing

UNITED STATES DEPARTMENT OF ENERGY

PUBLIC HEARING

ON

LAWRENCE LIVERMORE LABORATORY

ENVIRONMENTAL IMPACT STATEMENT

Multipurpose Room
Granada High School
400 Wall Street
Livermore, California
April 12, 1979

APPEARANCES

John Farmakides, Chairman
Board of Contract Appeals
Department of Energy

L.T. Grose
Professor of Geology
Colorado School of Mines

Victor Beard
Professor of Physical Chemistry (Ret)
University of Utah

Joe La Grone, Manager
San Francisco Operations Office
U.S. Department of Energy
Oakland, California

Byron Murphy
Sandia Laboratory
Livermore, California

Jim Olsen, Plant Services Manager
Lawrence Livermore Laboratory
Livermore, California

Dick DuVal
Assistant Manager for Programs
San Francisco Operations Office
U.S. Department of Energy
Oakland, California

I N D E X

I N D E X (CONTINUED)

P R O C E E D I N G S

MR. FARMAKIDES: Good morning, ladies and gentlemen. It's now 9:00, time for this hearing to be in order. This is a public hearing convened by the Department of Energy to receive your comments and views on the Draft Environmental Impact Statement of the Lawrence Livermore and the Sandia Livermore Laboratories.

That statement was issued in September, 1978 to assess the environmental impact of continued operations of the Laboratories and assess the alternatives to those operations. After the statement was issued the Department of Energy requested and received written comments on that draft impact statement following which this hearing was called by formal notice to provide a further opportunity for public comment.

The Livermore staff has prepared a statement in response to the comments which will be useful in focusing the issues for discussion and examination at this hearing.

The notice for the hearing was published on March 2nd, 1979, at 43 Federal Register 11821. It set out the purpose of the hearing, the procedures to be followed, and designated this presiding Board for this hearing. For the record and for your information, let me introduce the Board at this time. On my left is Professor Trowbridge Grose, a geologist and a professor of geology at the Colorado School of Mines. On my right is Professor Victor Beard, a physical chemist, and

formerly a professor at the University of Utah. He is now retired. My name is John Farmakides. I'm an administrative judge with a background in science and technology. I am Chairman of the Board of Contract Appeals, Department of Energy.

The notice also announced that this hearing would be a legislative type hearing, not an adjudicatory one. Therefore, we will not use formal procedures, such as testimony under oath, cross examination of participants, and so on. However, we will permit questions, provided they are relevant to the draft statement which we are now considering.

If anyone wishes to raise a question, please do so by writing out your question on a sheet of paper or on a card. If you don't have paper, there are cards in the back of the room that you could use. When you write out your question, please also write in your name and your home address so you can be properly identified in the record of this proceeding. Also, when you write out your questions, pass them forward, please. If someone would just simply bring them up to us, I'd appreciate it.

We'll receive them and ask the questions of the appropriate person on the staff. Or if the question is addressed to a participant, we will ask you to answer, if the question is relevant.

Please remember that in becoming a participant, each person also consents to answer questions raised of him or her.

The Board will review all the hearing record and in accordance with our charter, we will identify those issues which are unresolved and which we believe to be critical to the future decisionmaking involving the Livermore Laboratories. We will then prepare and present a report on these issues to the final decision makers. We're not chartered with resolving any of the issues. Our responsibility is to focus on the concerns of the public and to identify those issues which are critical and then to report them.

I think we can do so with the benefit of your views and comments and with your cooperation. We have read all the comments submitted and are now interested primarily in having your further views, especially those that address the staff summary that has been issued in response to your comments.

It would be sincerely appreciated if you can be concise in offering your comments. In fact, if you have a written statement, we would appreciate it if you could summarize the written statement and allow us to place the complete document as an exhibit to the record. If the statement is one which the participant wants to read in full, and if it's two, three or perhaps four pages, please give a copy to the court reporter so that she can have it and be better able to follow you. It gets rather difficult following a statement without something in writing before you.

Also for the record, when you are to speak, please

state your full name and your address. If your name is long and somewhat complicated, like mine, please spell it out. It helps the court reporter again, and it will be appreciated. All of us benefit by having such a clear record, and we would appreciate your cooperation.

Incidentally, please be assured that we will take whatever time is necessary to identify the issues that you have and to be sure that the record reflects that. On the other hand, it's a waste of time -- and I'm sure you'll agree with me -- if we hear the same issues repeated time and time and time again. So if someone has already expressed your issue and that person has done an adequate job, please consider stating that fact, stating that you agree with that person's assessment, and that you would then consider simply giving us the written statement that you have so we can put it into the record. There's no need to repeat detail time and time again. Once the Board focuses on an issue and we evaluate its importance as being something that we would definitely put into our report, there's no need to develop the issue over and over again.

Now let me outline the schedule of the proceedings today. I will first ask the Livermore staff to provide you with a summary of the background and purpose of the Livermore Laboratories. We will then open the record to the comments and views of those members of the public who have asked to

participate. As a matter of fact, several persons have asked to appear early this morning. So in order to accommodate those schedules, we'll take them first.

And just to have some order here, each time we introduce a speaker, I will announce the names of the next two or three succeeding speakers so that you'll be primed to know that you're on next or subsequent.

Also, a couple of housekeeping matters: Please, there's no smoking in the auditorium. It's very disconcerting to those who do not smoke. We would appreciate it if you would smoke outside. Also, the lavatories are outside, marked "boys" and "girls."

For restaurants -- we'll go until noontime, and I assume that will be between 12:00 and 1:00. Restaurants in the area -- I don't know the area very well. I'm advised that if you make a right turn as you go out the front, go down to the corner and make a right turn, in about two or three blocks you'll run into a couple of restaurants. They aren't too terribly far away.

Let me also note, finally, that copies of this record that we develop this morning, including the Draft Statement that I'm sure most of you have, all the comments that we receive today, the transcript made by the reporter, whatever exhibits we receive, will be available to the public at various locations in document rooms. And those are

identified in the Federal Register notice. The closest are the Visitors' Center at the Lawrence Livermore Laboratory and the Energy Information Center in San Francisco.

We're ready to go now, insofar as I'm concerned. These preliminary remarks are all that are necessary from my end. We're here really to have your comments. I will ask the Livermore and the DOE staffs to introduce themselves for the record, and then proceed to give us an overview of the Impact Statement. I understand that Mr. La Grone will be the opening speaker.

STATEMENT BY JOE LA GRONE
DEPARTMENT OF ENERGY, SAN FRANCISCO OPERATIONS OFFICE

MR. LA GRONE: Thank you, Mr. Chairman, I am Joe La Grone, Manager of the Department of Energy, San Francisco Operations Office, which is located in Oakland, has local DOE responsibility for the operations at the Lawrence Livermore Laboratory. The Sandia Livermore Laboratory, located on an adjacent site at Livermore, comes under the jurisdiction of DOE's Albuquerque Operations Office. However, because of proximity, the San Francisco Operations Office was given the lead responsibility for preparing the draft environmental impact statement assessing the environmental impacts associated with operations at both laboratories, and the resulting document was called the statement for the Livermore site.

The Lawrence Livermore Laboratory was established in

1952 as a second nuclear weapons laboratory, the first one being at Los Alamos, New Mexico. Both of these laboratories are operated for the Department of Energy by the University of California.

Sandia Laboratories was established at Alburquerque in 1949 to conduct the engineering, research and development associated with nuclear ordnance. In 1956, Sandia established a branch at Livermore to provide closer working relationships with the Lawrence Livermore Laboratory. Sandia Laboratories is a wholly owned subsidiary of Western Electric Company and is a prime contractor of the Department of Energy.

Although the prime mission of the Livermore site has been and continues to be nuclear weapons and development, additional programs include magnetic fusion research, bio-medical studies, laser fusion, laser isotope separation, geothermal and solar energy programs. In fact, about 40 percent of the work at Livermore is energy development related. Approximately 7,000 people are employed at the Lawrence Livermore Laboratory and about an additional 1,000 people are employed at Sandia Livermore.

Soon after the Lawrence Livermore Laboratory was established, it was recognized that an additional site was required for high explosive tests in connection with the nuclear weapons program at Livermore. Such a site had to be in a remote area, but as near to the laboratories as possible. These

requirements were best met by the Corral Hollow site, later called and called today Site 300. Itis located in the sparsely populated hills of the Diablo Range about 10 miles southeast of Livermore. Experiments with high explosives have been conducted there at Site 300 since 1955, and that site covers an area of about 7,000 acres.

Prevention of off-site effects of laboratory operations has been a concern since the founding of the laboratories. An effluent control program that places maximum emphasis on controlling effluents at the source has, therefore, been in effect at the Livermore laboratories since the beginning of operations. Environmental monitoring is conducted to ensure that this program is indeed restricting the release of effluents from Livermore and Site 300 to levels well below applicable standards. Techniques used in this environmental monitoring program have sensitivities usually capable of detecting radioactive and non-radioactive pollutants well below environmental background levels. The program includes the collection and analysis of air, soil, water, sewer effluent, vegetation and milk samples. Environmental background radiation is measured at numerous locations in the vicinity of the laboratories.

Each spring the Lawrence Livermore Laboratory submits a report to the Department of Energy containing the results of environmental monitoring for the previous year. This report covers both of the DOE laboratories. The report contains

a summary of results, a description of the methods used and the results of all measurements in the form of tables. Maps show the location of all samples collected. Copies of the reports are sent to the Environmental Protection Agency, the State of California Department of Health, the California Regional Water Quality Control Board and the City of Livermore. These reports are public documents and can be examined at the DOE Public Reading Room, which is located at DOE, San Francisco Office, at 215 Fremont Street, or at the Visitors Information Center at the Lawrence Livermore Laboratory. Copies of these documents also can be obtained on order from the National Technical Information Service, at the U.S. Department of Commerce, 5285 Port Royal Road, Springfield, Virginia 22161.

Emphasis on off-site --

MR. FARMAKIDES: Excuse me, sir.

MR. LA GRONE: Yes, sir.

MR. FARMAKIDES: I'm sorry. There is discussion going on, and we can't hear very well. I'd appreciate it very much.

Sir?

MR. LA GRONE: Thank you.

Emphasis on off-site environmental monitoring should not be interpreted as a disregard for the health and safety of employees on site. Providing for health and safety of employees is a requirement specified in contracts for operation of all DOE facilities. Because the nature of the activity

requires work with a variety of chemicals, both organic and inorganic, and with radioactive substances, safe operating procedures have always been a fundamental and well-recognized standard operating necessity. At Livermore, experts representing all safety disciplines provide safety guidance to both employees and management in planning, establishing and maintaining a low-risk work environment. Safety teams monitor all Livermore operations to detect and evaluate hazards. Emergency response personnel are trained to control accidents or emergencies. Research is continually conducted in such areas as fire safety, radiation detection and protection, chemical hazards and respiratory protection. During the history of the Livermore site, there have been four fatal job-related injuries. All of these resulted from vehicle accidents, three of which occurred off-site. Falls or falling objects striking personnel are the most frequent cause of lost time accidents. Less than one percent of the lost time injuries are caused by toxic material or radiation.

Throughout the year, DOE specialists from the San Francisco Operations Office and the Labuquerque Office visit the Livermore Laboratories to review and evaluate the respective contractor's performance in such areas as general management and technical program operations, nuclear material controls, waste management, security, health, safety, fire protection and environmental protection. DOE headquarters specialists

frequently participate in these evaluations.

There is a significant difference between the draft environmental statement that is being reviewed at this public hearing and most statements. Environmental impact statements are generally prepared prior to a proposed federal action, usually a specific project or program, and are prepared to identify environmental impacts that could result from the contemplated action, or to consider alternative courses of action and to furnish a basis for selecting these options that will mitigate or at least minimize the adverse environmental consequences. However, in the case of this environmental statement, we are reviewing an on-going operation that began in 1952. Thus, identification of potential environmental impacts and what was judged adequate mitigation was already taking place prior to passage of the National Environmental Policy Act.

In response to the National Environmental Policy Act of 1968, an Omnibus Environmental Assessments of the Livermore sites were completed and placed in the public document rooms at the San Francisco Operations Office and the Albuquerque Operations Office. Subsequently, a notice of intent to prepare this statement was placed in the Federal Register on July 16, 1976. Comments and suggestions were solicited. One comment letter was received in response to that notice, and that one concerned traffic congestion on East Avenue. This issue was considered during preparation of the draft EIS.

In September 1978, the draft environmental impact statement on the Livermore site was distributed for public review and comment to federal and state agencies, local governments and other individuals and groups known to have an interest in the sites.

A total of 24 comment letters were received from government agencies, organizations and individuals. A staff statement has been prepared in response to these comments and the concerns expressed were placed in eight general categories. A staff statement has summarized the various categories and provides information on how we propose to respond to these comments in the final EIS. Copies of the staff statement were sent to those who prepared written comments, and others who requested them, and they are available here today.

Some of the issues that were raised were considered to be beyond the scope of this environmental document which is intended to be a site specific analysis. Therefore, it should be recognized that the scope of this draft impact statement is limited to addressing the site specific environmental impacts of Livermore operations, alternatives to site operations and trade-off analysis between the costs of environmental impacts and the benefits derived from continued operations at this time. It is not intended to discuss issues related to national defense policies such as maintenance of a nuclear capability.

We are here today to give Federal agencies, the state,

other organizations and individuals further opportunity to present their views on the Livermore draft EIS and proposals for the preparation of the final EIS. The draft EIS was prepared by an interdisciplinary team of technically qualified specialists, some of whom are present today, to attempt to provide answers to your questions.

We will give fair consideration to all viewpoints on the environmental impacts of continued operation at the Livermore site. It is our intention and expectation that we can speak to some of the issues that will be raised. We do recognize that we may have to restudy some issues and possibly initiate new studies in order to adequately address some of your concerns.

Perhaps the most prominent concern expressed in comments we've received thus far centers on the effects of an earthquake on the Livermore site. There are several parts to this issue. One part has to do with identifying the maximum credible earthquake that we should be protecting against, as well as the probability of its occurrence. Another part has to do with identifying the consequences of that earthquake at the site.

We have spent considerable effort in both areas. We analyzed the geology of the area ourselves, and we hired a private firm to do the same. These efforts have been reported in the draft EIS. And based on these studies we have instituted

measures for mitigating the consequences of what was identified as the maximum credible earthquake.

However, as a result of new seismic information about this region, we will do new studies to verify the maximum credible earthquake. Some work will be started soon, and we hope to do further work in cooperation with the U.S.G.S.

We have done seismic analysis of our critical buildings based on results of the earlier seismic studies, and pending results of the new studies we have also begun further analysis of these facilities.

In conclusion, I believe we have been responsive to the requirements of the National Environmental Policy Act, and the reasonable concerns expressed thus far by others who have participated in this process. Further, we will continue to evaluate potential environmental impacts of our future operational and will do what is within our power to minimize and protect against those potential impacts. The comments we have received thus far on the draft EIS will be of help to us in this regard. Therefore, we wish to express our appreciation for the time and effort, to those who spent time developing those comments.

These remarks that I have made will, of course, be a part of the written record. But, for those of you who are interested, copes are available here today.

At the table with me, representing the Sandia Livermore

Laboratory on the far end is Byron Murphy; representing the Lawrence Livermore Laboratory is Jim Olsen, and, also, representing the Department of Energy today and serving as the principle spokesman during the proceedings that will occur today for the Department of Energy is Dick Du Val, Assistant Manager for Programs at the San Francisco Operations Office.

Thank you, Mr. Chairman, for the opportunity to make these opening remarks.

MR. FARMAKIDES: Thank you, sir.

I was also handed a letter from the Board of Supervisors, from Valerie A. Raymond, Chairman, and this refers to the draft environmental impact statement. The essence of the letter -- it is three paragraphs -- occurs in the second paragraph, and I will read it into the record. "The Alameda County Board believes that the present draft contains insufficient information as outlined in the attached reports from the Alameda County Planning Commission, the Environmental Protection Agency and County staff, and requests that these comments be considered and responded to in the final environmental impact statement."

I will enter this letter plus the additions into the record, and will ask, then, that the staff, in fact, do this in the final environmental impact statement.

We haven't had a chance to read this yet, so I just don't know what it contains. But, as the hearing progresses, I will have a chance to read this in greater detail later. So

this will be Exhibit No. 1 in the record.

Now, there were, as I said earlier, a number of people who asked to participate early this morning. I have the following names: Luke Ellis, Mr. Luke Ellis; Mr. John Saemann, S-A-E-M-A-N-N; Mr. Wilson Talley; Mr. Daniel Ellsberg. We can go in that order if you like. Mr. Luke Ellis? Is he here, please? I'll come back again later on that. Mr. John Saemann, S-A-E-M-A-N-N? Mr. Wilson Taley, T-A-L-E-Y, Mr. Taley?

STATEMENT OF WILSON TALEY

MR. TALEY: Mr. Chairman, sir, I'm Wilson Taley. My address is 1 Clipper Hill, Oakland, California, 94168.

I've watched the progress of environmental impact statements since 1970 when, as a Special Assistant in the Office of the Secretary of HEW, I got to review the first two that came through the Department. Since that time, and especially as assistant administratory for research of the Environmental Protection Agency, I've seen another dozen, two dozen come through. It has been a slow learning process on all sides of this matter. I would say that there has been a gradual improvement in the documents, but, unfortunately, their length has increased faster than they have improved.

I've read the draft environmental impact statement, and I would say that it is a respectable document. There are some minor errors of fact which I'll submit for the record by letter.

I would like to comment on one matter. I recognize that by the law, the reading of the law, and by history, these documents have tended to address matters outside the boundaries of the project or the program that is going on. They have not traditionally been directed toward the occupational area. Yet, I believe, a description of the Lawrence Livermore Laboratory's employee monitoring system would be a valuable addition to the final environmental impact statement.

As it stands now, the EIS leaves the impression that the perimeter of the sites are the first monitoring stations, when, in fact, the monitoring of the 7,000 employees at Lawrence Livermore Laboratory and the Sandia employees is really the first element of the monitoring system. This may seem a trivial point, but my personal experience in dealing with the Keypone tragedy in Hopewell, Virginia made me sharply aware of how much protection can be afforded to the public by the establishment and maintenance of such a system. I believe that the impact, the environmental impact statement would benefit greatly from a full and complete description of the employee monitoring program, not only for radiation but for chemical hazards.

As I say, I will submit, for the record, errors of fact ranging from moderately important to the absolutely trivial.

That concludes my statement at this time.

MR. FARMAKIDES: Mr. Talley, from your experience,

sir, how do you guage the length of this particular statement? In the record, we have quite a few comments that the statement is inadequate, insufficient. What is your analysis, sir?

MR. TALEY: Having spent three years deaing with documents of this type, I would prefer, if any errors are made, that they be on the side of brevity. At least it allows you to go through the whole document in one sitting, read it, and discover any errors or inconsistencies. It is always possible to expand a document. I found it very difficult to trim it down and have it remain coherent.

MR. FARMAKIDES: Thank you, sir. Did you have any questions that you wanted to pose?

MR. TALEY: Not I.

MR. FARMAKIDES: Thank you very much, Mr. Taley.

Let's go back again and repeat. Mr. Ellis, Luke Ellis, Mr. Daniel Ellsberg, Ms. Carol Bowen. Ms. Bowen, can you come up here now, please?

After Ms. Bowen will be Ms. Dawn Williamson, Ms. Annemarie Henderson, Ms. Edna Williams, Mr. Anthony Berriro. Please excuse me if I don't pronounce the names correctly. You can correct me when you come on the record.

STATEMENT OF CAROL BOWEN
U.C. NUCLEAR WEAPONS LABORATORY CONVERSION PROJECT.

MS. BOWEN: I'm here as a member of the U.C. Nuclear Weapons Laboratory Conversion Project today. And, also, I

don't quite know the procedure. I have a statement from Congressman Ronald Dellums that --

MR. FARMAKIDES: Is it very long, ma'am?

MS. BOWEN: No, it is --

MR. FARMAKIDES: You may read it into the record if you would like --

MS. BOWEN: That's what I would like.

MR. FARMAKIDES: -- or you can introduce it as an exhibit. You may summarize it, whatever you wish to do.

MS. BOWEN: Maybe I would like to read it into the record.

MR. FARMAKIDES: All right.

MS. BOWEN: The Statement for the Department of Energy Hearings regarding the Lawrence Livermore Site, the DEIS, from Congressman Ronald V. Dellums, 8th District, California.

I'm deeply concerned by the draft environmental impact statement for the Livermore site and submit this statement to share with you some of these concerns. This DEIS is defficient in a number of areas, due primarily to the very narrow boundaries within which the statement is framed. Because of this narrow focus, there is no serious consideration of a number of problems which could affect the health and safety of Bay Area and northern California residents and have the potential to damage or destroy our local environment and, perhaps, a more wide-spread area.

The statement fails, for instance, to address the problems of radioactive waste disposal in a comprehensive manner, only suggesting that site containment is adequate. What, however, will ultimately become of this waste? When the bulk of radioactive waste will be with us for several hundreds of thousands of years, surely, it can be considered to have an environmental impact, and, certainly, the Department of Energy should be concerned with the proper permanent disposal of waste from the Livermore site should such disposal even prove feasible.

In this regard, we should note the many problems with disposal outlined by the inter-agency review group on nuclear waste management and the critique of their report by a number of state agencies.

The draft statement is also deficient in dealing with the consequences of the major release of radioactive material and in attendant civilian emergency. The definition of DOE responsibility in such a situation is defined in the narrowest of terms, leaving other governmental agencies to cope with the monumental impact of a major accident.

My office learned recently, for example, that in one of the cities in the 8th Congressional District, there has been but one meeting of the Disaster Council for the last two years, and that no effective plans exist for dealing with this type of nuclear disaster despite the real risk posed by this University of California Laboratory. While we can hold local officials

responsible for such negligence, certainly DOE bears the prime responsibility in this matter to make certain the public clearly understands the devastation which could ensue in the event of a major release of radioactive materials. The DEIS seems the appropriate arena to explore these consequences.

More immediately, however, I'm particularly concerned by the recent discoveries of additional earthquake faulting in the Livermore valley. As you may be aware, my office has been an intervenor in the NRC hearings regarding the GE test reactor at Vallecitos in the Livermore valley, also. And we have seen there are numerous earthquake faults which only recently have been mapped or properly identified for the potential intensity of a major seismic event. There is, in my estimation, an urgent need to undertake a thorough investigation of the faults on or near the Livermore site.

I understand that DOE agrees, and I want to take this opportunity to urge that this be a major independent study of the Tessler and other faults in the area to determine the full extent of the danger they pose. If the Tessler fault is longer than currently mapped and does run beneath the laboratory facilities at Livermore, the implications are manifold for possible danger to the health and safety of Bay Area residents.

In the interim, there exists considerable danger in the form of large quantities of plutonium which are stored and used at the Livermore site. Were there to be a major earthquake

that caused the release of this plutonium into the atmosphere, the consequences would indeed be catastrophic. In addition, other evidence to be presented here today indicates there may already be a grave danger to the surrounding population from an increased risk of cancer.

I request, therefore, that until such time as the potential danger of these newly discovered earthquake faults and other problems can be adequately assessed, all plutonium at the Livermore site be removed to a storage facility or another site removed from the threat of earthquake or other potential damage.

MR. FARMAKIDES: May we have a copy of that letter? If we could have the original, it would be great.

MS. BOWEN: Okay.

MR. FARMAKIDES: Are you representing the congressman?

MS. BOWEN: No. This is an exhibit. He couldn't be here and didn't sign up, so --

MR. FARMAKIDES: All right. We will receive that into the record as Exhibit No. 2.

Do you live in the area?

MS. BOWEN: I live in Berkely.

MR. FARMAKIDES: Could we have your address for the record?

MS. BOWEN: Yes. 1315 Delaware Street, Berkeley,

California.

MR. FARMAKIDES: Thank you. Did you have something else that --

MS. BOWEN: I'd like to say a brief statement from the U.S. Nuclear Weapons Labs Conversion Project.

We were formed two and a half years ago to try to address the many problems that are caused by both the Livermore and Los Alamos Laboratories. And, in all that time, we have called many times for an independent environmental review of both the Livermore and Los Alamos Labs. We first addressed our concerns to the University of California, who, supposedly, manages these labs and got absolutely no response from them.

We were pleased to have the Department of Energy eventually get around to issuing a DEIS. And in December we critiqued it and issued a nine page statement detailing our criticisms of the DEIS. And we were instrumental in seeing to it that other individuals, concerned individuals commented on the DEIS. And, as a result of the many comments that were received, this public hearing has been held. And we are very pleased to see this. Because it seems that it is a very dangerous facility in many ways, both local and global dangers are represented here, and there has been very little public interest in looking into these problems until the last few months.

There is supposed to be a public health study that the state assembly has mandated, and this has not been -- well,

it was supposed to be done by this May and it has just been started and not yet been completed, and we want to encourage that a much more intensive health study be done on the Labs. The DEIS is rather deficient into the human environmental problems that are caused by the large amounts of plutonium and other radioactive materials that are dealt with at this Lab.

I think that is all I want to say. We have other speakers coming later in the day.

MR. FARMAKIDES: Thank you very much. If you could kindly give that letter to the court reporter, I would appreciate it very much.

MR. BEARD: The question that I would like, and I am not sure this is appropriate. I would like a response from the Livermore Laboratory concerning plutonium, and the storage and the precautions taken. I'm not sure that we ought to wait for the complete elaboration of what they feel the problem to be before we get the response. But I think very definitely we do need a response concerning earthquake, storage, et cetera.

MS. BOWEN: From the Laboratory?

MR. BEARD: From the Laboratory.

MR. FARMAKIDES: This seems to be the thrust of your concern.

MS. BOWEN: It is. And we've had -- if you people could elicit some information out of them, I think it would be a tremendous public service. It is impossible for local people

to find out information.

MR. FARMAKIDES: This does you, the local people, the opportunity of asking questions. And if we feel, if this Board feels that they are relevant to the statement, we'll have those questions answered.

MS. BOWEN: I see.

MR. FARMAKIDES: Could the Livermore people -- Mr. Du Val, could you kindly respond to that?

MR. DU VAL: Of course, the points you've raised were raised by others with regard to the earthquake question and are the subject of a number of the comments in the staff summary. I do not want to take up this group's time repeating those.

With regard to the earthquake situation in the Livermore valley, that has been the subject of a series of examinations dating back some number of years, and is a matter of continuing study. And, as Mr. La Grone mentioned in his opening statement, will be the matter of further study, so that we can increase our knowledge and, on a continuing basis, assure ourselves that we are operating in a safe mode with regard to those activities.

MR. FARMAKIDES: Mr. DuVal, I think it would be appropriate if you have people with you, or yourself -- while it has been addressed in the staff's response, I think for the benefit of those people here who have not read that, you might want to go into detail as to the concerns expressed by this

young lady, storage of plutonium, the safety factors involved, the type of facilties that you have for --

MS. BOWEN: Also, I would like to -- Mr. DuVal just pointed -- mention, in the course of this comments, that he is interested in assuring -- the Lab is interested in assuring themselves that everything is safe and done is a proper manner.

We are also interested in getting some information for the public other than just the Lab assuring itself that it is safe, and this has been an on-going problem that we just -- well, the staff comments, they simply just --

MR. FARMAKIDES: Well, insofar as that is concerned, that is the whole purpose of this hearing. Because, very frankly, this is information that the public -- all the comments are in the public record. The response to the comments are in the public record and, certainly, everything developed here to-day is information going to the public. So the response of Mr. DuVal right now is going to be a public response.

Mr. DuVal, could you, please?

MR. DU VAL: I would like to ask Mr. Olsen, the Plant Manager at Lawrence Livermore Laboratory, to give us a summary with regard to the question you raised with regard to plutonium storage and with regard to the earthquake question.

MR. OLSEN: I'm Jim Olsen, the Manager of Plant Services.

We, at the Laboratory, recognize the point that you

have made. We are convinced that the plutonium is handled safely, but we recognize that there is some concern in the public. To try to remedy this, recently we have had a number of public officials in the cities of Livermore, Pleasanton tour through our plutonium facility in an attempt to show them how we operate.

Now we are unable to state the total quantity of plutonium we have on site because of classification. We can, however, state that the quantities of materials that we have in a work status are relatively small; that is, we work with four kilograms in any one work status. So most of our material is stored inside of a vault. Now the vault is constructed of reinforced concrete; it's very substantial. This building that we work with the plutonium has been designed or upgraded to withstand a .5G thrust. All right? Now a building that is --

MS. BOWEN: That is not all right.

MR. OLSEN: The building that is designed to withstand that load will actually take a much higher load with appropriate safety factors that are in the design.

Now the material that is stored inside of the vault, where most of the material is, is inside metal containers, doubly contained, and stored inside of what we call bird cages. This makes it such that even if the packages are shifted around or moved, they cannot fall into a configuration that will give you criticality problems. Now, inside the vault, there are also

no gasses or any other flammable materials. Also, all of the shelving and the containers are bolted down to take any kind of an earthquake thrust.

Now the work that we do with the plutonium and the work status is done inside glove boxes, and the glove boxes have not air inside them but arogon gas. The rooms and the gove boxes have double filtration from high efficiency filters. So we feel that these areas are well taken care of. And there have been at this time no accidents inside of our plutonium building that we have not been able to contain completely inside the buildings.

MS. BOWEN: The one question I have, in the staff comments there is some mention of --

MR. FARMAKIDES: Could you address your question up here, please?

MS. BOWEN: Sorry. That accidents have happened, and they said that there is going to be a further -- in the final environmental impact statement -- a further enumeration of the accidents that have taken place. We are very concerned about those accidents. And, also, it seems the problem is not with the plutonium that is stored in the vault, the larger problem, and there might be some problem there, but it is with the plutonium that is out that is being worked with that could have the possibility of becoming airborne plutonium, which is what can cause cancer. And it only takes one pound, sort of

properly despersed in the atmosphere, to cause -- to potentially cause cancer for everybody in the world, lung cancer if it is inhaled. So that assurances that the stored stuff is not going to be critical is not sufficient to meet our concern about the possibility of those four kilograms in all the different glove boxes.

MR. FARMAKIDES: So then your question goes to the point of is there any way that the plutonium can escape the building into the atmosphere and into the soil?

MS. BOWEN: Right. And what measures are they taking. I mean we are trying to find out information.

MR. FARMAKIDES: An awful lot of that information is already in the record, ma'am. I've read this, and I've read your comments. There is an awful lot that is already in there that is answered.

We will ask the staff again to respond briefly, if you can, to this point.

MR. OLSEN: Let me repeat again. The way we work with plutonium is inside of a glove box. Now this glove box has inside of it an atmosphere of arogon. The glove box pressure is less pressure that the room in which you are working. Now the glove box is tied down to the ground. It is protected with fire sprinklers. So, if an accident occurs inside the glove box with the plutonium, that accident will be contained within the glove box. Now if something occurs to cause it to

get outside of the glove box, it will then get into the room. But that room is at a lower pressure than the outside hallways. And that room -- the air that goes through that room goes through double filters, so that the plutonium that is airborne will stay within that room.

Now if it then gets out of the room into the hall, the hall is filtered. Now the building will withstand the design basis earthquakes in our area. And we have those rooms fire-sprinklered. And we also have, if the plutonium catches on fire, we have the proper fire retardant material to cover that.

Now the other thing that you should recognize; that there are no high explosives in this building. So that the worse situation could be either a fire or a criticality. Now if a fire occurs with plutonium -- if you burn a block, a four kilogram block of plutonium, it burns and forms a slag over it. And the most of that four kilograms that you could get airborne is of the order of a couple of grams. It is similar to molten metal. When we have molten metal, all of the metal does not get into the air. It is a very small part. And the filters will adequately filter all of that out.

MR. BEARD: May I ask a question then for my clarification? I would like to come back to the earthquake hazard and ask one or two questions. I would guess the box is criticalically safe with respect to flooding, as well as with -- your response

is it is criticalically safe with respect to flooding.

I would like one or two other responses.

MR. FARMAKIDES: I think the response was yes. We have to speak because we are building a record here.

MR. BEARD: Let's come back to the glove box now, and let's postulate an accident. I take it that the vents have proper flexible joints or stiff joints so that you can stand with respect to getting your vent system, the right kind of shaking? I'm not saying how much, but they are designed -- are they designed so that you have flexible joints on this system?

MR. OLSEN: Yes. The system is designed to take the design base earthquake, so that they will still be there following the earthquake.

MR. BEARD: One other question with respect to the glove box. The phase that could be shaken loose around, where the glove box are, are they properly tied down?

MR. OLSEN: Yes. The glove boxes and the equipment in the glove boxes are tied down.

MR. BEARD: So a -- couldn't get loose, you would presume, and smash into the glove box and rupture them?

MR. OLSEN: No. All of the material is tied down in the building, shelving --

MR. FARMAKIDES: Dr. Grose had a comment.

DR. GROSE: Ms. Bowen, you raised a point of concern regarding your feeling that .5 g is not adequate. Is that

correct?

MS. BOWEN: There certainly is a question of where that .5 g came from and why -- it is just stated that .5 g is sufficient for this site. And, as far as I understand, and there are going to be people later in the day that are more seismic experts, and I don't want to go into it.

DR. GROSE: You don't wish to pose that as a question now then?

MS. BOWEN: No.

DR. GROSE: All right.

MS. BOWEN: So when somebody comes up who is ready.

MR. FARMAKIDES: I think that is correct. There are a number of people coming on who are apparently seismologists and geologists and they will probably talk to that point.

MS. BOWEN: Yes.

MR. FARMAKIDES: Thank you very much, Ms. Bowen.

Let's go back again. I think what I will do is that I will announce the names three times. Then if the names are not here, then we'll drop them. And if they come in later on, please come up and see me.

Again, we will go back to Mr. Luke Ellis. Is he here? Mr. Daniel Ellsberg? Ms. Dawn Williamson? Ms. Annemarie Henderson? Ms. Edna Williams? Mr. Anthony Beriro? Mr. Steve Ladd? Mr. Ladd.

MR. FARMAKIDES: Would you kindly identify yourself, sir, for the record.

STATEMENT OF STEVE LADD
WAR RESISTORS LEAGUE WEST

MR. LADD: My name is Steve Ladd, and I'm speaking here today as a representative of the War Resistors League West.

MR. FARMAKIDES: The last name is spelled L-A-D-D?

MR. LADD: L-A-D-D, that's correct. And I believe that I wrote in and requested 20 minutes earlier, is that correct?

MR. FARMAKIDES: Yes, sir.

MR. LADD: Do I have that 20 minutes?

MR. FARMAKIDES: Yes, sir.

MR. LADD: I hadn't known that I would come on so early, so I am just getting my materials together.

MR. FARMAKIDES: We started off initially, sir, giving the early morning hours to those that had requested the morning and then we just went in order.

MR. LADD: Oh, I see.

MR. FARMAKIDES: I have a couple of lists here; I just took the first list and -

MR. LADD: Oh, that's fine. Let me just begin by saying that I really come with no particular expertise in the area of nuclear physics or health sciences or seismology or any of the other questions that will be addressed here today by others with

more expertise, but I come with a deep concern about the kind of work that goes on at this Lab, how that work is conducted and its environmental consequences. Before I address some specific comments about the DEIS, I'd like to put my remarks in some context. I think that the recent events around Harrisburg, the Three Mile Island Plant, have helped me and I think many other people in this country in this area, to crystallize a number of very important feelings. I think the central one of that is particularly that I, and I think many others, no longer have any faith in the so-called experts of the nuclear establishment, if indeed we ever did. Up to the day before the Harrisburg accident I am sure that the people responsible for the plant would have said, and in fact may have said, that this kind of accident would never happen, could never happen. We've heard that same kind of talk about other nuclear power installations and similarly we've heard comparable talk about the Livermore Site, as I think is exemplified in the DEIS. Now that the controversy of whether there is real danger at Harrisburg is seemingly over, or whether there was any danger ever before due to the release of radiation, we now hear government officials and nuclear establishment officials and the like telling us that there is really nothing to worry about now that the radiation dangers will cause no problem. We also hear the voices of independent reputable scientists like Rosalie Bartels, Ernest Sternglass and others, who have conducted studies

linking radiation to increased cancer rates, telling us otherwise. Who are we to believe in this situation? Those with some kind of vested interest in protecting and continuing the nuclear industry, or those who are independent of that establishment and have the interests only of public health and safety in mind? I think these recent events along with others such as the refutation of the Rasmussen report, the lack of progress in coming up with permanent waste storage sites and the various studies coming out that link exposure to nuclear materials to higher cancer rates have caused a healthy distrust of the so-called nuclear experts. To put it bluntly, the credibility of these experts is on the line. Similarly, the credibility of the statements made in the DEIS, the staff response, comments by Lab officials and even this presiding Board are credibilities all on the line.

I think that the public has for too long become the captive of a scientific. technological elite that has promised us a better future, a better defense, a more secure existence. Only lately have we begun to wake up to the fact that this elite which President Eisenhower so eloquently warned us against in his farewell address is a short-sighted and often self-serving bunch whose motivations may be economic, personal gratification, discovery for discovery's sake, other reasons above some of the potential human consequences of the actions. Perhaps now in the light of recent events we can challenge once and for all

the notion that the scientific technological elite, and here specifically the nuclear elite, knows what is best for us and indeed are somehow acting in our best interests.

With this in mind, I address the following comments and questions, specifically to the DEIS on the Livermore Site. Generally my comments fall into three categories. The first is a belief that the public has a right to know the full extent of the potential hazards that Lab operations pose. Excuse me, that will be the second. The first is, we need to be absolutely certain that the DEIS, the staff statement and the final EIS are thorough unbiased studies of the potential and real hazards at the Lab Sites. And finally I have a number of assorted important questions which I do not feel are addressed at all or adequately in the DEIS.

In regards to the first area of concern which I mentioned second, I have reason to believe that the DEIS is not a thorough, independent, unbiased study of the real and potential hazards at the Labs. I have a number of questions in that regards which I feel should be answered. For instance, who are the people who put the study together? What are their names? Who are the people who wrote the staff response to the public comment? What are their names? Who will write the final EIS and on what basis will it be decided to accept or reject certain criticisms or suggestions for action? I have asked these questions of the DOE and in a written response that I received

from Mr. Pennington I feel that the responses were inadequate. Mr. Pennington said that in response to my questions about who wrote the EIS that, he said, "It's not been our practice to indicate who wrote EIS's." Now, one press account of the DEIS that I read mentioned a Dr. Arthur Toy of Livermore who was involved in writing the document. Were there others involved from the Lab? How many? What was their role? Who else was involved in writing the DEIS and the staff responses? What have been their past associations in the DOE, ERDA or AEC or other nuclear-related agencies or industries? It seems to me that the answers to these questions are critical if we are to look at this report as credible and we are to have any faith that the final EIS will hold any hope for substantive changes to decrease the hazards the labs pose. Are there people of the critical calibre of Drs. Bartel, Sternglass, Dr. Johnson who is here today, Dr. Mancuso, Goffman, Morgan and others who strongly disagree with current nuclear standards. Were they involved at all in this process? Or people like them? If not, why not? If you want this to be a credible report, I suggest that such people be directly involved, not simply as participants in a public hearing like this today, where you can discard their testimony later in some private discussions among yourselves - and I don't know how that procedure works either - but as full participants in the decision-making, as to what actions should be suggested in the final EIS. I think we know

we can no longer trust the nuclear establishment; so how can we be expected to trust a report with no list of authors, that, through some mysterious process and a private process, gets written and rewritten.

In a recent hearing before another DOE committee where I testified, which was a sub-group of the Energy Research and Advisory Board, Dr. Solomon Buxbaum, Chair of that Board, criticized the UC Nuclear Weapons Lab project for not having the names of the authors of its report on the University's operation of the Labs on that report. And when I told him later that the Department of Energy issues reports similarly with no authors like the DEIS, he said that he felt that that was inadequate as well, and he criticized that process.

I also think it would be helpful for all of us to know in breaking down and demystifying this process how you all were chosen for this Board, what some of your backgrounds are, and exactly what process you use to make decisions about this; how are decisions made about some of the comments being brought forward here today? I think in line with this too - I hadn't known who would be sitting here as well to answer questions - that it is interesting that Livermore Lab officials are answering questions about their own safety, which seems to me to call credibility in question as well. I want independent experts.

Second area of my concern, I feel that the DEIS has not provided nearly enough information to the public in a number

of significant areas. For instance, I think it is absolutely critical that the EIS include a full listing of what radioactive materials are routinely used, stored or transported at the Labs and in what forms and amounts. Now the question of amounts has already come up, but three years ago a reporter for the KPFA Environmental News Service questioned a then ERDA official in Oakland about what amount of plutonium was at the Lab, and he was told there was approximately 600 pounds of plutonium at the Laboratory, with some variations in amount on site at any one time. But when people have subsequently asked that question, they received the answer which we've gotten here today already, that that information is classified. I want to know why that information is classified, what is being protected by having that information classified? Surely the Russians don't care what amount of plutonium exists at Livermore. Are the people that are really being protected perhaps the people who live around the Lab, that the information is being hidden from? It seems to me that a complete assessment of the potential health hazards at the Lab or the Lab site in Livermore, cannot be complete without a full listing of the kinds and amounts of nuclear substances in use there.

Similarly, I believe that the public has a right to know what routes the transportation of nuclear wastes and other materials in and out of the Labs take, so that they may be alerted to the potential dangers of a nuclear accident in their

neighborhood or community and can decide whether they wish to continue having radioactive materials transported through their streets. Are we to presume that the Labs and the government have some God-given right to force the transportation of these materials on a populous, without the knowledge of the people who might someday be affected by that?

In addition, I think that the DOE staff statement did not adequately answer the question of whether plutonium flights will be resumed into Livermore. The statement merely says that, "At present the Livermore site has no plans for routinely shipping plutonium by air." Does this mean that there may be such plans in the future? The word "routinely" implies that they could happen anyway. What is meant by "routinely"? Were shipments before the development of a new cannister considered routine? Reference to the transportation of nuclear materials I found it odd that, in Section 5.3.1 of the DEIS, entitled "Partial Relocation", that the alternative of moving some of the critical buildings at the Lab to someplace like the Nevada Test Site is rejected in part because "a greatly increased need for movement of material between the NTS and Livermore would expose the public to possible transportation accidents." But nowhere in the DEIS is there any reference to the possibility that a transportation accident could occur in the present operation of the Lab, which is also involved in a great amount of transporting of nuclear materials.

I also think it is important for the public to know if there are radioactive materials used in constructing nuclear warheads such as plutonium that have been unaccounted for, and if so, what the reasons are believed to be for this.

Additionally, I think it is important to know if there have ever been any security breaches involving a possible theft of such materials, and what the response has been.

I would like to move on to the third and final area of concern which includes a number of other questions that I don't feel were adequately dealt with or dealt with at all in the EIS, particularly in areas which you might call off-site impact of lab operations. The staff statement indicates that only "sites' specific environmental impacts can be considered," which seems to mean that anything that happens off-site or far from the lab as a consequence of lab operations might not be considered in the EIS. I would like to know under what regulations governing compliance with NEPA that the authors of the staff statement base this comment. In my readings of the regulations governing DOE's compliance with NEPA, I fail to see where such a limited definition can be found. One possible kind of EIS mentioned also in NEPA compliance - or compliance with NEPA, DOE's compliance with NEPA - is what's called a program EIS to analyze the consequences of developing certain kinds of technology not on the site, but if that development were put into use, what would be its impact in the general

public. And in another section of the NEPA guidelines, it states that, "EIS's covering a site under DOE jurisdiction such as major research laboratories or production facilities shall assess the individual and cumulative environmental consequences of a number of continuing and/or proposed actions at the given site." Now it does say site there, but it does not say "on the site" - it says rather "at the site," implying that this is where the continued or proposed action takes place, not where the study of environmental consequences should be limited to. So I'd like to know where that particular citing comes from.

To get into some of what I would like to bring up in regards to off-site impact, other than the obvious potential disastrous global impact of a nuclear war, and whether the technology being developed at the Lab is leading us closer to that possibility. I'd like to focus on two areas where Lab work already has had, I believe, a tremendous off-site impact that I think should be assessed in the final EIS to make it a responsibile document.

First is in the area of nuclear wastes. In terms of cumulative effects of the Lab's operations, it seems essential to me that there be a full accounting of how much nuclear waste has been generated over the years, what kind of waste has been created, and particularly, where all the nuclear waste from the Labs has been stored, dumped or buried. This seems particularly

critical to me in light of the public disclosures about the 50,000 barrels of waste that originated at Livermore and at the Berkeley Labs that have been dumped off the Farallone Islands near San Francisco and have since begun to leak. Where else have these wastes been dumped that might be hazardous to the populous? That is not in the EIS. And further, how can the Labs continue to generate wastes when there is no permanent waste storage facility, and there appears to be nothing in sight for the permanent, unquestionably safe storage of waste for up to 250,000 years for plutonium? What will be the long-term impact of this waste and its storage? The DOE indicates that the Livermore site cannot consider this question of nuclear waste generated at the site. On what regulation governing NEPA compliance is this based? And what other agency will take up this assessment if not the DOE and through this DEIS?

Similar logic I think holds for the impact of nuclear testing, both above ground and below ground, conducted by the Livermore Lab at the Nevada Test Site and elsewhere. I've read the DEIS on the Nevada Test Site and nowhere does it deal with the recent revelations about the impact of nuclear testing on nearby residents, particularly in Utah, which have come into public light lately. As you are no doubt well aware, recent studies have shown a definitive link between some of that fall-out from nuclear testing and Leukemia in children in parts of Utah. Others exposed to nuclear testing who have since developed

cancer are now suing the government for damages as well. And Dr. John Goffman has done a study that suggests that up to one million people may die from plutonium-induced lung cancer in the Northern Hemisphere as a result of that fallout. Even underground testing, according to various studies, has posed a real health hazard. In the first eight years of underground testing - from 1963 to 1971 - according to a report by former Senator Phillip Hart, thirty per cent or a total of twenty-eight of those tests vented and vented sufficient radiation to be considered a major release, releasing radioactivity from two hundred to one million curies per explosion, according to the Livermore Lab itself. That upper figure is comparable to the fallout from one Hiroshima bomb. Twenty-three of those tests, according to a March, 1979 report by Paul Duckworth, a former nuclear engineer and NTS employee, were conducted by Livermore. How and where does this environmental consequence of the work at the Lab get analyzed? The NTS EIS does not cover it, nor does the Livermore site EIS. Is it perhaps that no one really wants to face up to the consequences of what the Labs have been doing? I call on you to include an assessment of the environmental impact of the Livermore-conducted nuclear tests in the Livermore site EIS as one of the cumulative and continuing consequences of the Lab's operation. If it is not included in the EIS, I urge that you reopen the EIS on the Nevada Test Site in light of new information which, according

to my reading of your regulations you can do if there is new information - and I believe there is - and seek wide public comment and hold a hearing on the subject in the very near future.

Finally, I have two other comments. I'd like to know on what basis you do program EIS's to determine potential adverse impacts of developing certain technologies, as I think I've indicated I read in the NEPA compliance regulations. I'd like to know because I believe it's important that there be an assessment of at least two of the Lab's fusion programs. You only briefly mentioned the laser and magnetic fusion programs in the DEIS and you mentioned them in a positive light and mentioned none of the criticisms which have emerged among the scientific community. For instance, particularly you ignore the weapons application of the laser fusion program which are documented in an April, 1977 Science Magazine, nor do you mention the allegations by Ray Kidder of the Livermore Lab and founder of the laser fusion program that a midterm application of the program is to create a fission fusion hybrid reactor that would create plutonium as a by-product, nor do you cite an aritcle by Dr. John Holdren of the University at Berkeley in the Bulletin of the Atomic Scientists that many of the underlying concepts in the fusion energy program, if they become more available, may make it easier for other countries to develop nuclear weapons, nor do you mention criticisms again

in the Bulletin that the laser isotope separation program at Livermore may make bomb-grade materials cheaper and easier to obtain. Since you mention many of these programs, though, in the EIS, it seems that the responsible thing to do would at least be to mention some of the potential adverse impacts in the final EIS. And I'd like to know further again on what basis a program EIS would be conducted on these programs at the Lab.

I had a number of other comments here, but I think I'll just wind up with that, because I think I've gone on fairly long. And I'll submit the rest of them in written testimony. But I'd like to know particularly by what process some of the questions that I've raised here today would be answered. Thank you.

MR. FARMAKIDES: Thank you, Mr. Ladd. You obviously have a number of questions to raise and, are you going to submit these you say to us? When will you do that?

MR. LADD: Well, this copy is rather rough at present. I can retype this and submit it to you, if you like. I don't -

MR. FARMAKIDES: How soon?

MR. LADD: In the next couple of days.

MR. FARMAKIDES: Well, I'll tell you what. How much time do you need until? Saturday? Monday?

MR. LADD: Well, I could type this up tomorrow.

MR. FARMAKIDES: Could you?

MR. LADD: Yes.

MR. FARMAKIDES: You want to mail it to me?

MR. LADD: I could do that.

MR. FARMAKIDES: All right. If you, why don't you mail it to me, Board of Contract Appeals, Department of Energy, Washington, D.C.. Now what I will do is send those questions to the Livermore people, the DOE people in San Francisco, and ask them to respond to you directly. Now what I will also do is put yours into the record. And their response to you into the record. That leads me then to the only question that I think I can address, we can address. The function of this Board really is to interface with you and the staff, the Livermore staff and the DOE staff here, and to see what are your concerns, in view of the draft Environmental Statement and in view of the response of the staff to your comments. What are your further concerns. We then will evaluate your concerns from the point of view of, have they been answered? Have they been treated? Have they been adequately treated? We feel in our best judgment that they have not been adequately treated, either in the draft or in the summary. We will mention these in our report and we will ask that they be treated. Our report then becomes part of the official record, so that in fact this entire process is on record. Your comments, of course, they are on the record. Now, who we are. Well, obviously Dr. Gross and Dr. Beard are outside members of the

public, consultants, if you will; they are not DOE employees. I am the only DOE employee. One is a seismologist - I'm sorry - a geologist, and the other one is a physical chemist and nuclear engineer. Both have had experience in their fields, wide experience in their fields. My background, as I said at the outset, is legal. I do have a science background. Why I was picked? Primarily because I do not answer to the Secretary of Energy, except in the sense he can fire me. My decisions are reviewed only by the Court of Claims, and my decisions are reviewed by the Federal District Court, so there is no decision that I write that is reviewed by anyone within the Department. I assume that the reason why I was chosen was because of that point. I haven't been fired yet, so I assume that, as a matter of fact I say that in jest - my decisions are reviewed on appeal by people who receive a decision from mine, and I always write my decisions so that I alert the people against whom I'm finding the full reasons why I do that. They can appeal it. That is the reason that I think I was asked to be on this particular Board, and I have been on a number of these boards. The Rocky Flats Board was an earlier board on which we participated. Dr. Gross was on that board. And again, the reason he was on that board is because of the geological questions raised. Okay, that gives you an idea of who we are and what the process is in terms of ourselves.

Now, once the report that we prepare is completed,

that report will be sent up to the decision-makers with respect to Livermore. We are not the decision-makers. All we do is focus on the issues that you people present, and we will then give that report to the decision-makers. Now, who they are in this case, eventually it's the Secretary of Energy. Coming down from there, I just don't know who else will become involved in the decision-making. In the final analysis, the Secretary of Energy is the one responsible for the decision-making.

MR. LADD: Does he appoint another Board to consider the questions raised, or what? How does that work?

MR. FARMAKIDES: Consider, the questions raised today?

MR. LADD: Yes, what is the exact process by which he finally decides on these comments?

MR. FARMAKIDES: Well, the comments that we will come up with based on what you people give us are placed into a report which becomes a public document. That report, along with the draft, along with your comments, along with everything else, is then the package upon which -

Excuse me just a minute. I'd appreciate very much if we can possibly hold off just a bit - can we get off the record for just a minute.

(Off the record)

MR. FARMAKIDES: Anyway, this entire package is then part of the public record which will be used by the decision-maker. We are not that. We are not that. Now your questions

again. If you will please submit them to me by Monday, I'll wait three days or so, because I assume there will be some mail lag time, and then I hope to receive it. Okay?

MR. LADD: Can you give us an idea whether the rest of the process will in any way be public or we will know who the final decision-making people are, and can you give us an idea of when your report will be put together, whether that will be, when that will be available.

MR. FARMAKIDES: Yes. Our report - we have a target date among the three of us - our report hopefully thirty days to forty days. But I would think we would have all the materials, we would have the opportunity of looking at the materials, your materials and also the summary statements and also responses to your questions and coming up with the report within thirty days, thirty-five days, something in that ball game. Dr. Gross is in Golden, Colorado. Dr. Beard is in Utah, and I'm in D.C., so it is also - that causes a little bit of a problem, but not much. It generally takes a little while to sit down and think through the comments made and decide which of these comments really are critical comments that have not been addressed. Or if they have been addressed, they are so critical that in fact they should be further considered. For example, we've gone through these comments in great detail, and we already have ideas as to what we consider to be critical issues. The seismology of the area obviously is an issue that

we think is critical. And we will discuss this in depth. Okay. Is there anything else, sir?

MR. LADD: Well, the only other thing is, you've given a background for yourself, which I think has been helpful and I appreciate that. I think it would also be helpful if the other members of the Board did that as well, so we can establish what some of their associations have been and where they're coming to. I understand they have particular expertise, but I think, as I mentioned earlier, some of the questions that I think have been raised through recent events are, who can we trust? What associations do they have? Are they really independent experts? I would like to believe that, but I'd like to know a little more about their background. I think others probably would too. And your comments, as I said, I think have been helpful.

MR. FARMAKIDES: I'll tell you why I hesitate. There is implicit an assumption which you've just said that bothers me. And that is that this Board is not credible, and that the only people that are credible are those people that you agree with. I hope that's not the case.

MR. LADD: No, I'm just suggesting, I don't know, I respect you all as individuals, but I don't know where you're each coming from and at least it would be helpful, I think, to know whether there is somebody who has been independent of the nuclear establishment and usually when there are studies done

that are of a critical nature and the public is really watching, I think it's helpful to have at least one recognized critic of many standards who the public can be assured that somebody is speaking on their behalf, even if that person doesn't constitute - singly, obviously, wouldn't - a majority. But it's helpful at least to know that many of the concerns expressed by the public or large segments of the public, I believe, would be considered, and not simply in a public hearing, as I think I indicated.

MR. FARMAKIDES: Well I can appreciate your concern. And I will ask Dr. Gross and Dr. Beard to very briefly state their background. But I hope you do mean what you say, that you're not attacking the integrity of the Board, because very frankly, I think we're sitting here learning from you, and you people are the ones who are looking over our shoulders, for example. The record that we compile is your record; it's a public record. And there's no way that anything that comes out today is not going to be in the public record.

MR. LADD: Right. Well I think that's very much appreciated, like I said.

MR. FARMAKIDES: Dr. Gross, would you give a little bit of your background, sir?

DR. GROSS: Mr. Ladd. I've been a professor of geology for twenty-five years. Essentially that's my major activity. Nine years at Colorado College, Colorado Springs.

Sixteen years at the Colorado School of Mines. Most of my time has been involved in the academic world. However, since 1954 when I was a student at Stanford, I have been consulting, a consulting geologist, consulting for industry, for government, consulting very widely in the petroleum industry, in the mining industry, in the environmental fields, geotechnical engineering, geological field. I've been involved in scientific research, in techtonics and regional geology in the Western United States proving Andes (?), Australia, supported by the government, National Science Foundation, USGS and so on. I have no particular ties to anyone, never have had. I'm essentially a professor and an independent consultant for wide varieties of industry and governmental concerns. Is that sufficient?

MR. FARMAKIDES: Thank you. Dr. Beard, could you just give a -

Oh, there is one more thing, Dr. Gross. Dr. Gross, as I said earlier, was on the Rocky Flats environmental hearing along with myself. Dr. Beard?

DR. BEARD: Well, as is obvious by looking at me, I'm an old man, so I have been involved in alot of things. I received a Bachelor's Degree at the University of Utah, Ph.D. at Purdue University, with a fair amount of graduate work at Cal Tech. My specialities for a long time were thermodynamics and quantum mechanics which I taught at the University for a long time. Along well into that career, some ten years into that

career the whole field of atomic energy opened up and there was no way that you could get into the atomic energy field unless you actually became a consultant to the government. That's where we all came from. There was no such thing as nuclear engineering; we had to come in as the various skills. I did work for the old Atomic Energy Commission for eleven years. I did then pick up some, I think, some expertise in nuclear engineering because I subsequently taught that at the University of Utah. For the last eighteen years - oh, I left the Atomic Energy Commission I guess mutually. It was a mutual sort of thing. I was happy to get out and get back to school teaching. I think they were probably happy to get rid of me. But nevertheless one tries to be objective, so if I am tainted by some knowledge of nuclear and if I had to learn it, about the only way that an old man can learn it was back in the days when the Commission was doing this. The last eighteen years I have I hope been independent and detached and devoted to teaching. And that's about where it is. I did participate with Mr. Farmakides in the Idaho hearing.

MR. FARMAKIDES: Mr. Ladd?

MR. LADD: Well, I thank you. I'm sorry to take up so much time - it's been very helpful -

MR. FARMAKIDES: That's all right. We did it because, as you said, I think there are alot of other people in the audience that also would like to know. Thank you, sir, very

much.

MR. LADD: Thank you very much.

DR. BEARD: I have one question - this bothers me. Actually, our only job is to try to reflect your concerns and get an answer. Your concerns on radioactivity, and this is important to me to try to reflect your concerns, most of them had to do with freezon (?) material, but you said all radio-active material. Now, you're smart and intelligent, you can tell the way you talk, you know that potassium 40 occurs naturally and of course it's in all the food that you bring in and actually it's a waste product - it's in the urine and feces that you give out. So obviously you don't mean all the radio-activity. Can you give me a feeling for what quantities you are interested in? Microcuries, pico curies, what - or do you want to say all radioactivity not naturally occurring?

MR. LADD: Perhaps that's the best way to state it. Yes. As I mentioned, I don't have an expertise in nuclear physics, so I'm not that well versed on specifically what I should be asking for, which I think is one of the problems with the public in matters like this.

DR. BEARD: Well, I'm trying to find out. I'm trying to be helpful.

MR. LADD: Right, no I appreciate that. I would defer possibly to others who might come up later who might answer that better, who have some other -

DR. BEARD: I'll put in my notes here all non-naturally occurring radioactivity. Will that satisfy you for the time being?

MR. LADD: For the time being.

DR. BEARD: All right. That's all.

MR. LADD: Thank you.

MR. FARMAKIDES: Thank you, Mr. Ladd. Let's go back then for the third time I will repeat the names of those I called out earlier. Mr. Luke Ellis, Mr. Daniel Ellsberg. Mr. Daniel Ellsberg. Mr. Ellsberg, you're on, sir.

STATEMENT BY DANIEL ELLSBERG

DR. ELLSBERG: I am going to speak -- these are not questions that concern experts exclusively, by any means. That's why we're here. I am going to begin with some comments on my background here, because I'm going to address myself to a relatively narrow point, but a very important one that I think would otherwise be left out. So the kinds of things I want to raise today are not things that are determinative and they really can't be answered here entirely. But I want to be sure that they are not ignored in these hearings.

My career, after I left Harvard was systems analysis at Rand, strategic analysis at the Rand Corporation in Santa Monica, specifically work on the command and control of nuclear weapons and the design of strategic nuclear war plans. It came to be that in 1961 I drafted the Kennedy Administration guidelines for Secretary McNamara of the Strategic Nuclear War Plans of the United States. It was a very great responsibility at that time.

Let me ask a question, with all respect, to set the framework here a little bit: whether any of you gentlemen -- I would assume the answer is no -- have in fact seen a nuclear war plan or one of the then current nuclear war plans of the United States. Could I ask?

DR. BEARD: "Planes," did you say?

DR. ELLSBERG: Plans. Plans. Not airplanes, but actual plans for use.

MR. FARMAKIDES: I think we have not.

DR. ELLSBERG: Yes. I asked that question also of the Regents of the University of California who have administrative or formal responsibility for the work going on at Livermore. What happens at Livermore in the weapons research and at Las Alamos too, two places that have designed all of our nuclear warheads, is to meet in a broad sense, requirements that are set by national policy in one administration after another, and specific requirements by the Pentagon through the Atomic Energy Commission or the DOE.

Without knowing the current administration's concept of what plans these weapons are to implement, one does not have any specific notion of why the weapons are being produced. If in an Environmental Impact Statement one is supposedly weighing costs against benefits, as I believe the act requires to be done, it's really not adequate to push off the benefits side with a simple statement, "We assume these are essential." Indeed, if we assume that, there's nothing more to be said. It doesn't matter what the risks are, and the costs. And that's really been the spirit in which this problem has been approached, I think, for a long time.

Unlike nuclear energy for which there are substitutes, supposedly here we have an area of national security for which

there are no substitutes for these weapons, and we just have to accept whatever risks.

I repeat the question, which I don't regard as going without saying. We know there are some health hazards to these kinds of operations with plutonium. We know, of course, there are some risks of accident. We can argue about how large, and how many people are affected.

For what purposes are these risks being incurred by the people of this community and people of much larger communities, if there should be larger accidents? That can only be answered if you know what the nuclear weapons are for. I not only have some direct knowledge of that, but because of that I know very well that that knowledge is very, very narrowly held, and that the likelihood that you people with the responsibility of looking at this question would be able to answer it correctly is very small.

It's also true that the Regents of California, when I asked them, although they all -- or I believe most of them, at least -- have clearances in order to carry out their responsibilities -- they did not say that they had ever had a briefing on this subject. This included President Saxon.

I was able to get only so far in the few minutes -- this far in the few minutes that I had on the two occasions I've spoken to the representatives of the University. I said, "Here is my background. I believe the question is important.

I offer myself, for what it's worth, and I'll give you what I know. And you can pursue it for further investigation." But that was never picked up.

I'm glad to have been given -- at least I was told it had been arranged because of our prior request -- to be given some more minutes here, so that I can go into the substance that the Regents did not choose to ask for, on what these weapons are for.

MR. FARMAKIDES: Well, let's be very clear --

DR. ELLSBERG: You'll see immediately why it bears. I think you'll see immediately why it bears.

MR. FARMAKIDES: That would be something I would ask you for, sir, is to connect it to this Statement.

DR. ELLSBERG: Yes. Precisely.

MR. FARMAKIDES: Just be very clear: We're not about to make the decisions that you're suggesting.

DR. ELLSBERG: No.

MR. FARMAKIDES: We're not charged with that type of responsibility. So really and truly, I don't know that your point is complete. I think all we're doing here is focusing on the issues, like the one that you're suggesting now, and seeing if we can articulate those issues so that the decision-makers, whoever they might be -- and those decisionmakers may or may not have the experience that you suggest. Those decisionmakers are not ourselves.

DR. ELLSBERG: Precisely, but on the other hand, you and I are citizens of this country, which means we really are the decisionmakers.

MR. FARMAKIDES: Well, Dr. Ellsberg, I think that you're --

DR. ELLSBERG: I don't think that's a metaphysical point. What I mean is -- I don't want to exaggerate romantically our powers. You and I know -- we're old enough, and I'm old enough to know the limits, the apparent limits on those powers. I'm talking about responsibilities. And I'm saying to be a citizen in this country conveys responsibilities for informing oneself about the issues that affect all of us and our children that really, other citizens of other countries can manage to neglect and say it's really not their business. We can't say it's not our business.

I'm happy to have the opportunity today to talk to you as citizens, in the lives that you'll lead after you finish this post, who have an opportunity to share with the officials who work for you and us anything you may have learned, from me or anyone else -- critically from me, in reaction to me.

MR. FARMAKIDES: How much time do you need, sir?

DR. ELLSBERG: Well, they said -- I've used up, I know, in the credentials, about five minutes here, or something.

MR. FARMAKIDES: How much time do you want?

DR. ELLSBERG: I'd be very happy to have 25 minutes, 20 minutes. I mention that because we asked for half an hour, and they said we had it.

MR. FARMAKIDES: You state your point very well, that we do have responsibilities. No doubt about it. Our responsibilities here, members of this presiding Board, are to accomplish the taks that's before us. But you also recognize that we don't have the power to do what you're suggesting.

DR. ELLSBERG: I think when you hear what I have to say -- though you don't need to believe it -- but when you hear, as a basis for hypothesis, you will look for more power than you may think you have now. You may even find you have it.

MR. FARMAKIDES: Mr. Ellsberg, I'll tell you what we'll do. I think that what you're suggesting is really beyond the scope of this particular exercise today.

DR. ELLSBERG: Could you make a judgement a little bit further on, and when you see how relevant it is --

MR. FARMAKIDES: Yes. I was going to suggest the following: Let's give you, if you will, ten minutes. And we'll see whether we should continue beyond that. All right?

DR. ELLSBERG: Having taken the effort --

MR. FARMAKIDES: I want you to connect, really, your testimony to this Statement. That's why we're here today.

DR. ELLSBERG: Yes. Right. From past experience on

discussing this -- I'm not bargaining just over minutes -- but from what it takes to do some justice to this, can we say 15, and I'll go on?

MR. FARMAKIDES: All right. Go ahead, sir.

DR. ELLSBERG: All right. That's what it takes. That's not very long. I'll speak fast. And I want you to hear it.

MR. FARMAKIDES: No. What we're doing is building a relevant record. I want to be sure that it really applies to what's before us today.

DR. ELLSBERG: Very clear.

MR. FARMAKIDES: And the nuclear weapons policy of this country is not -- as far as I'm concerned, it's not before us today in this Draft Environmental Impact Statement.

DR. ELLSBERG: The nuclear weapons policy of this country, in terms of alternatives and choices, has never once been put before the American people, the sovereign public, nor were they ever given the information to generate the hypotheses or the alternatives. It has never been a matter of democratic public discussion -- understandable during the war against Nazi Germany, the Manhattan Project. But really, that was 34 years ago. And there has never been a time when the public was told what I'm about to say and the facts -- whether it's on the risks, the hazards, the costs, or anything else, to make democracy possible.

I propose, if I may -- without being too grandiose -- we start, or I start, at least, today doing that, right now -- make it a matter of public discussion. What has seemed to preclude the necessity for that has been the hoax throughout that period that the single, predominant, overwhelming purpose of the design of these weapons at Livermore and Los Alamos and elsewhere is to build weapons in order to deter a possible nuclear attack against the United States or its allies. When the word "deterrence" is used, is almost always heard as it is meant to be heard -- deterrence of a nuclear attack against the United States or possibly its treaty allies in NATO, Western Europe.

That has never once been the predominant purpose of those weapons or the function of them. There have been other purposes. And although one can argue as to the exact weight of various purposes, I think I can make myself clear when I say that has never been the primary purpose.

That purpose, I'll say -- and then put the point aside -- has been the one, the single one emphasized all this time, precisely because it is the one purpose that justifies the building -- justifies in virtually everyone's mind, including most people who think of themselves as pacifists, everyone but a very small percentage -- justifies building weapons that are by their nature and intent genocidal. And I don't use that word for rhetorical effect at all. I mean in terms of

the Nuremburg precise definition which we sponsored -- instruments designed or a process designed to kill all or part of an enemy populations. We are talking about weapons, the largest of which does the function of an Auschwitz, which killed 2 million people, if it's exploded near 2 million people, or 6 million, if it's near 6 million.

That has to be justified, especially to Americans. If it is to deter retaliation of attack, to deter attack, to respond to it, and thus to keep it from ever happening, that reason is regarded by most Americans, including myself, as justified. That's why we have heard that reason and no other.

Now I will say what the reason has been at various times -- the ones that I can speak to directly. In the years 1960 and '61, when I was reading and working on the war plans of the United States, we had -- I saw an estimate by Herb York the other day -- about 10,000 warheads. Ten thousand warheads. That's 20,000 less than we have now.

So for good or bad -- and I'll come back to that -- in the next 19 years we added to that 10,000 another 20,000. The Russians went up from very few at that point to the 20,000 or so they have now. So the world has seen about 40,000, as a result of reluctance by ourselves as well as the Russians, to stop that arms race.

What were those weapons for? I was working night and day at Rand because I, too, thought at that time -- and

then in the Pentagon -- that there was an imminent fear of a Russian attack, missile attack, on a capability that was predicted by the head of Strategic Air Command probably to mean 1,000 Russian missiles in the year 1961. That was his estimate by his chief of wär plans to me at Offit (?) Air Force Base, Omaha, in September, 1961 -- 1,000, now.

The other predictions prior to that had been several hundred. The estimates were several hundred. The reality was four. There were four Russian ICBM's in 1961. And although I'm sure -- you lived through that -- and I'm sure you did learn in '61 and '62 with the rest of us, that the missile gap had not come out as predicted. You have not seen the figure I just gave. And I think the figure you may have seen in books is 50, an order of magnitude higher. And the difference between a significant number of missiles facing 50 SAC bases or 25 SAC bases -- and no missiles.

So we essentially had something very close to a monopoly still in 1961, 16 years into the missile age, of intercontinental strategic power. And that had been known to our intelligence services during the late 50's when they were designing the plans that Kennedy signed in late '61, after that estimate was official, for 1,000 Minutemen. Four Russian ICMB's then. Some more began to be built in 1962. We started our program with 1,000 Minutement, adding to the 2,000 intercontinental bombers that we had. They had some 200 --

adding to the thousands of tactical planes, the two Polaris submarines by that time, the Atlas and the Titan.

Now, what was that all about? Not to deter a Soviet attack, because there was no capability for a Soviet attack on the United States. I've given you the exact figures. That's an approximation, but a pretty good approximation.

What then were the plans that I was working on for? They were quite explicit. We had one way in our plans under the Eisenhower New Look period, the '61 period, for fighting Russians. President Eisenhower had determined that we could not afford in our economy to build up conventional non-nuclear forces with which to challenge Russian forces anywhere -- or limited nuclear forces that could challenge them in limited nuclear war which could expand. We could only afford to fight Russians one way.

However the conflict started, and whoever started it -- and this was all explicit, whether the East Germans started it in a scuffle, the West Germans, whether there was something in Iran, whatever happened -- whether the Russians were in Iran, Cuba as they turned up in '62 in sufficient numbers to trigger this plan, or the Middle East, or wherever it should be -- that plan was for a first strike against the Soviet Union. By that I mean a first nuclear strategic strike.

It did not assume that we would strike out of the blue, unannounced, that we would relax them, that it would be

a total surprise. The presumption was that it would come out either of some kind of conflict or an imminent conflict, but not one involving nuclear weapons. The assumption was that we would be the first to use nuclear weapons, and we would not use them in a limited way, if the conflict involved Soviet and U.S. troops, as in the Berlin corridor.

I have now made precise what I mean. It was not preventive war in the narrowest sense of that term, a war out of the blue with no conflict. But it was for an initiation of nuclear war by the United States, and not only in limited terms, as around Berlin, but initiation which, out of a perhaps Berlin-type conflict, would take the form of hitting, simultaneously, every nuclear capable facility in Russia and China -- of course, there were none then -- and every city in Russia and China.

The calculated (by the Joint Chiefs of Staff) effect of carrying out their war plan, which was the single war plan -- and not merely a piece of paper, but a basis for buying weapons, designing them at Livermore -- all the weapons to be dropped would have been designed at Livermore and Los Alamos, all of them. Building them all over the country, sending them, deploying them all over the world, the bases, training -- I watched people take off on ten-minute alert.

Dr. Strangelove -- I reported in the Pentagon that year as an expert on command control. That film was a

documentary of what could happen, because my speciality was the possibilities of accident and unauthorized action. That was to implement a plan which in turn had generated those requirements for the weapons here.

That plan, as I say, was to respond to any conflict with the Soviet Union in this fashion. And when the President asked the Joint Chiefs a question that I had drafted, he got an answer. The question was -- in '61 -- the question was: "If your plan was carried out as planned, without being obstructed by the opponent, how many people would be killed in Russia and China alone?" Ignoring fallout outside Russia and China and ignoring their retaliation, and ignoring our allies' response.

They gave the answer: 325 million people in Russia and China alone. Adding in the factors that I've included, which involve by the way, by their calculations, destroying -- this was their answer to, let us say, a new Iranian crisis involving U.S. and Russian troops, or Middle East or Berlin -- I have to keep repeating that -- or a Taiwan Straights crisis, or a Korea in which Russia got involved, which we knew could happen. Our response then called for wiping out Japan by fallout alone. That was our calculation, wiping out Finland by fallout alone, Pakistan -- nearly every country of our allies adjacent to Russia would be wiped out by our strikes, ignoring Russian retaliation.

When you add in the various other factors, their retaliation and so forth, we're talking about 600 million people. That is the holocaust in Germany on a scale of 100 -- 100 times. And that was an American plan. That was my colleagues doing that. I was working with them.

That was not just a bizarre effect with our preoccupation with economy in the Eisenhower years. Not only have such plans continued -- although they are no longer the exclusive plans. The first thing I did in establishing new guidelines was to establish that reluctant though we were to fight Russians under any conditions, there were ways of fighting Russians that did not involve hitting Moscow. That sounds like a macabre joke. But that change had to be made. I'm sure we do have plans now that do not involve hitting Moscow.

You can also be sure that we have plans that not only involve hitting Moscow before we have been hit, circumstances like those of the year -- and that's the point I have to come to quickly now.

I'll pin it down with one reference which, although it's a best seller, seems to be known by no one in this country, the freest and with the best press in the world. Haldeman alleges in his memoirs what Nixon's secret plan, his boss' secret plan to win the war in Vietnam was. Quite apart from whether he's correct or not, I don't know anyone that I know who has read those memoirs -- you can raise your

hands in exception -- or knows the following allegation: He says that Nixon's plan was to end the war by making secret from the American public -- secret threats of the initiation of nuclear weapons if his terms were not met, and that he based that idea on the actual historical example of his former boss, Eisenhower, who ended the Korean War on acceptable terms by making such secret -- from the American public -- but explicit ultimatum to the Koreans, as I find Eisenhower says in his memoirs.

That allegation, then -- and he says that Kissinger delivered such threats in '69. I've made a study. I'm reporting it now at Stanford in a course I'm giving there now. I've pulled the references together. I'll be glad to give them to you. The documentary evidence exists by now -- in most cases, many years after -- for the following first-use threats by the United States Government, all secret from the American public at the time, or in cases where there may or may not have been explicit threats, readiness and preparedness to use nuclear weapons first, within days or weeks, by the United States: 1950 in Korea, '50 and '51; '53 in Korea under Eisenhower; '54 Dien Bien Phu in France. Nixon reveals that we gave -- I'm sorry -- that Radford offered -- Admiral Radford, Chairman of the Joint Chiefs -- offered the French three nuclear weapons to defend Dien Bien Phu. I was in the Marines at that time. The Marines were offshore at

Dien Bien Phu. I didn't know about this fact. The prime minister at the time, Bidot, has been saying for 20 years that he had been offered nuclear weapons. It has never been confirmed by an American official till now. Nixon puts in his memoirs the precise figure that Bidot gives -- three, one of them to be used against China. Indeed, in '69 one of the targets was a mile-and-a-half from China. And I know this from the person who worked on that plant, Roger Morris.

That was '54. In '55 they were deployed. We were ready to use them over the offshore islands in the Tejans (?). In '58 the defense of Quemoy was based entirely upon the plan to use nuclear weapons first, if necessary. I have the formerly top secret, now declassified report, giving that in great detail. In '61, in Laos. Of course, we know Berlin and Cuba, the only two cases that involved Russia, the only two public cases. In '68, Caisson, according to Westmoreland's memoirs and other studies based on interviews, our readiness to defend Caisson with nuclear weapons -- and '69 to '72.

Here then is the moral: Why has Livermore been in business all this time, since '52, a time when the Soviet Union -- when Livermore was established -- had no operational nuclear weapons at all, according to recent studies. It exploded a device in '49, as you know. Why has Livermore been in business ever since then? I would say to implement the actual use of nuclear weapons secret from the American

public that has gone on every year or two since then.

A gun is not used only when it's fired. When you point a gun at someone's head, when you brandish a knife at someone, you're using that weapon. It menaces, whether it draws blood or not. Every term of every president, from Harry Truman on, has seen as a matter of now public record, not known to almost any member of the public, has seen strong consideration by the president in the Oval Office of the imminent use in limited war situations against non-nuclear powers, not the Soviet Union, not in Western Europe -- the imminent first use initiation of nuclear weapons against a non-nuclear power.

They have used it, and I'm sure they have often felt they used it with success, although they didn't want to tell the public why they were running the risks of nuclear war over the Taiwan offshore islands, the Taiwan Straights, over Laos, et cetera, et cetera. But I think that's why they want them.

That does give a rationale -- and I'm coming to the end now. That gives a rationale for weapons like Trident, cruise missile, Missile X, neutron warhead, otherwise, by the way, not easy to explain. They, of course, try to explain them in terms of deterrence of nuclear attack. But the President, of course, destroyed that essentially, quite validly, by pointing out that one Posiedon submarine can cover -- designed here, to the technical credit of Livermore -- can put a nuclear weapon on every Russian city of over 100,000.

There are 218 such cities. The Poseidon submarine can hit 224 targets. That's one. We have 31 Poseidon submarines and ten other Polaris. Why, then, Trident? Why is Livermore still in business?

As I say, it becomes hard to answer. One reason -- jobs, votes. You'll hear that. I think this record gives another additional answer. There is a coherent function they form. The function is to support by backing up the first use threats with the smaller weapons, also designed at Livermore, that have been made over the last 30 years, to make sure that when we use those weapons against an ally of the Soviet Union that could be supplied with nuclear weapons in return, it will be a unilateral use. Otherwise, we would be assuring the destruction of the area we were supposedly defending. We want to make sure that the threat is credible so that we don't believe there will be a retaliation. To do that, you have to look very big -- not only bigger than our adversaries, like Vietnam. That's easy. Bigger than their ally, Russia. We have to look very, very big. You can't be too big.

In short, here is the choice facing the American public, if they understood the issue, and facing you. I'll sum it up now.

MR. FARMAKIDES: You're summarizing?

DR. ELLSBERG: Yes. If, now that you've heard it --

and by the way, you don't have to believe it, because you've heard it from me.

MR. FARMAKIDES: That's right.

DR. ELLSBERG: But you've heard it, and it's there now. You can ignore it and so forth. But let me -- I will summarize. Ignore it and go on --

MR. FARMAKIDES: No. I'm saying that you're expressing your point of view. And I frankly have no way of getting involved in --

DR. ELLSBERG: No, pardon me. Wait. That's not right. You are involved, sir. And I say that with all respect. You are an American citizen. You are a human citizen. The citizens of Russia, China, Britain and every other country in this world are involved. I'm not being rhetorical, but you really are involved.

MR. FARMAKIDES: Let's summarize, Mr. Ellsberg.

DR. ELLSBERG: Yes. The summary is this. The choice that you should be addressing is this, I believe, in my respectful opinion: The costs and the risks which are real -- and you will hear them from other witnesses -- of operating this facility and the production facilities in this country, are indeed, as in all cases, risks to be weighed against benefits. And those benefits are to be understood in the light of alternative objectives.

The objective which we have secretly, actually been

pursuing is the objective of a superiority which we don't have at the moment, but had in the past and hope to have again, officially. And I will say that if you accept the President's perspective -- not just one president, but all of them -- that we must continue not to be able to make first use threats, but that we must continue a policy of making first use threats and have the ability to make those threats credible and to back them up in Europe and elsewhere, all over the world, then we do need Livermore. I agree. And we do need Los Alamos, and they should go into three shifts a day, because we can't have too many nuclear weapons for that, and there's no end to an arms race.

If you accept as I do, what most Americans believe -- these aren't the only two alternatives. But most Americans believe the only legitmate and necessary function for us, which is the deterrence of nuclear attack -- then we had many thousands of nuclear weapons more than we needed 20 years ago. We have many more than 20,000 in excess now. We do not need Livermore to design new ones, and especially to serve this function.

MR. FARMAKIDES: So that really is your point, sir?

DR. ELLSBERG: That is my function.

MR. FARMAKIDES: Your point is that we no longer need Livermore, for the reasons that you've given.

DR. ELLSBERG: And I will make one more sharp point in

addition to that. We not only don't need it, but in the light of what I've been saying, if we recognize that keeping Livermore in operation and our determination to do so in pursuit of this policy, has kept us from making genuine and effective arms proposals for bilateral limitations, we have assured the proliferation of these weapons, not only in imitation in Russia, which is, I think, the major effect; but a secondary effect of proliferation. And the effect of that has been that the United States is much, much less secure.

We have lost national security in the 25 years or so in which it has been pursuing this. That was understandable. It is going on. And every year we would have been more secure, had we taken arms control measures which were possible that we didn't take, because we wanted to keep Livermore in operation. I think that's true today, still. I'm happy to answer any questions.

MR. FARMAKIDES: Thank you very much. You're very articulate.

DR. ELLSBERG: Thank you for the time and the opportunity.

MR. FARMAKIDES: I'm not quite sure that most of what you said pertained to the statement, but you did have some point that did pertain to the statement.

DR. ELLSBERG: Could I suggest -- have you seen Dr. Arthur Taplin's comments from the Natural Resources

Defense Council? Do you know that organization?

MR. FARMAKIDES: Yes, I do.

DR. ELLSBERG: In his comments on the Draft Environmental Impact Statement for Rocky Flats.

MR. FARMAKIDES: Yes.

DR. ELLSBERG: Because he makes, in reference to the Act under which you're operating --

MR. FARMAKIDES: Did you want to introduce that, sir?

DR. ELLSBERG: I could, if you don't have it, because he points --

MR. FARMAKIDES: Why don't we introduce that into this record, Mr. Ellsberg?

DR. ELLSBERG: Yes, I will do that, because he makes the point which suggested to me that it was precisely your responsibilities to look at both benefits and, by the way, possible unnoticed costs on our own security of operating. I believe that this operation reduces our security, which is a stronger way of saying it is not necessary.

MR. FARMAKIDES: We'll take that as an exhibit, sir. Do you have any questions?

DR. BEARD: I have a question. I want it off the record. It's kind of a facetious question.

MR. FARMAKIDES: We'll go off the record.

(Off the record.)

MR. FARMAKIDES: I will go on, then to -- oh. There was one other gentleman who wated to appear in the morning -- Mr. John Saemann, S-A-E-M-A-N-N. Mr. Saemann?

STATEMENT OF JOHN SAEMANN

MR. SAEMANN: My name is John Saemann. I live in Alameda. I'm a concerned citizen. I have a prepared statement, sir, if I may read it.

MR. FARMAKIDES: Is it a very long one, sir?

MR. SAEMANN: No. It's about five minutes.

MR. FARMAKIDES: Yes, sir.

MR. SAEMANN: Well, parenthetically, may I say that this is a very inopportune time for me to appear. I think that Dr. Ellsberg said everything that I wish I could have said as eloquently as he has said it.

My presentation is much more limited, because I thought this hearing was mainly about the Environmental Impact in the region, rather than the political aspects.

MR. FARMAKIDES: It is the former, sir. It is the Environmental Impact of the continued operations of the Livermore Laboratories. Dr. Ellsberg suggested that he could connect his comments to that statement. We gave him 15 minutes to connect them. His connection was tenuous, but we permitted it.

MR. SAEMANN: Thank you. Someone once said, if you don't see it, it won't hurt you. We all know that that

isn't true anymore. Going back to invisible and odorless carbon monoxide and coming up today with submicroscopic plutonium, this and other dangerous radioactive elements and isotopes have so easily found their way into our everyday language that we no longer feel uncomfortable with them.

Let us never forget why plutonium was named after Pluto. My Webster's Seventh Collegiate Dictionary defines Pluto as "the god of the dead."

Testimony is being heard today that one active earthquake fault runs right through Lawrence Livermore Laboratories, and there are about a dozen others nearby. What will happen to the highly dangerous toxic substances used here when an earthquake hits? What has already happened when these materials are accidentally spilled and then flushed into the city of Livermore sewer system which eventually will end up in San Francisco Bay when the new system is completed. Then it will be too late.

The San Francisco Aqueduct is also nearby and its proximity represents a great danger to the people in the West Bay.

I am gravely concerned about the venting of radioactive gases and the distribution of invisible radioactive dust carried down into our agricultural fields in the San Joaquin Valley. The self-monitoring done on the premises of Lawrence Livermore Laboratory and its stations nearby,

although probably well-intentioned, can only be self-serving at the most critical times. It is like appointing the fox to guard the chicken coop. It's not good enough for my health or your health or the future of our children or grandchildren.

We have heard from years from experts and we have been told, "You can't understand this," or "It's in the national security interest. It's a secret. Just leave it up to the experts." The soldiers at Yerpa Flat, Nevada left it up to the experts back in the 1950's. And we've forgotten about the partial melting of the core of the Chalk River, Canada reactor in 1952; the 1957 runaway reaction and fire at Windham, England; the EBR core meltdown at Idaho Falls, November, 1955 and the January, 1961 reactor explosion there when three men were killed; the October 1966 Fermi reactor at Detroit which was within one second of exploding; the 1970 Hanford loss of coolant accident; and the largely unreported nuclear accident in the Soviet Union east of the Urals so large in devastation that our surveillance satellites showed it to the CIA which kept it covered up for many years; the Brown's Ferry, Alabama fire in 1975, and need I mention the recent Three Mile Island near disaster -- all without the additional hazard of the proximity of 13 earthquake faults.

I didn't address myself to the ultimate contamination, use of these infernal weapons designed at Lawrence Livermore

Laboratory, each of which makes us in the world less, not more secure.

I call upon the Department of Energy, sir, to live up to its name and make this facility an energy designing facility instead of a life-destructing facility. For the good of this community and the talented staff here, let us end the work with dangerous substances and redirect their efforts towards safe, life-supporting endeavors and new energy systems, instead of the present work with this plutonium for Pluto, the god of the dead. I am also a member of the University of California Nuclear Weapons Lab Conversion Project.

DR. FARMAKIDES: Thank you, sir. The next person is Don Williamson, then Miss Ann Marie Hendrickson, Miss Edna Williams. Is Miss Williamson here?

MS. HENDRICKSON: I'm Ms. Hendrickson.

DR. FARMAKIDES: Hold fast for just a minute. Is Ms. Williamson here?

MS. HENDRICKSON: She's not.

DR. FARMAKIDES: Oh. You know that for a fact?

MS. HENDRICKSON: Yes. They're all members of our group.

DR. FARMAKIDES: All right. You're next, then.

MS. HENDRICKSON: Yes. Friends of the Earth has offered to exchange some of their afternoon time in exchange

for some of our morning time. They would like to give their presentation now. So I would like to ask that Glenn Barlow of Friends of the Earth be allowed to use 15 minutes of our time, Edna and Don's time, since they're not here. They're also members of our group.

MR. FARMAKIDES: Well, I think we can do better than that. We'll just simply substitute them for yourself, and then we'll put you on later. All right?

MS. HENDRICKSON: All right.

MR. FARMAKIDES: What are their names?

MS. HENDRICKSON: His name is Glenn Barlow.

MR. FARMAKIDES: Yes. I see him on the other page. All right. Is Mr. Barlow here?

MR. SCHWARTZ: Excuse me, Mr. Chairman. My name is Schwartz. I was told I was scheduled to be the very first speaker at 9:00. Now, I am quite prepared to wait all day, but I think there may be a number of other people who did have a commitment to get through this morning. I wonder if you would please take the trouble to find out. I really am afraid that --

MR. FARMAKIDES: Mr. Schwartz, the only people that were called to my attention as having a morning commitment were the three that I read initially.

MR. SCHWARTZ: I've been told that some others, I think Mr. Riggin and Mr. Johnson had a.m. commitments. I

wonder if you could just ask of them to make sure that we don't lose someone important.

MR. FARMAKIDES: Mr. Barlow, would you mind if Mr. Schwartz went first?

MR. BARLOW: Could I explain?

MR. SCHWARTZ: I'm not asking for myself.

MR. FARMAKIDES: Mr. Schwartz, hold fast, please. Mr. Barlow, you've got the floor. You go ahead and take your time. We'll get back to Mr. Schwartz during the recess. I'll talk to Mr. Schwartz.

MR. BARLOW: I'd like to explain that I'm just going to take 15 minutes right now, and that I'm a member of the group Survival without Nukes who was scheduled to speak now, and this time period is substituted for the two women, Dawn Williamson and Edna.

MR. FARMAKIDES: In other words, you're talking --

MR. BARLOW: I'm speaking for them.

MR. FARMAKIDES: You're talking for Miss Williamson and who else?

MR. BARLOW: And Edna --

MR. FARMAKIDES: Edna Williams. All right, sir. Won't you proceed? Could you identify yourself?

STATEMENT BY GLENN BARLOW

MR. BARLOW: Yes. My name is Glenn Barlow, and my address is Box 377, College 8, U.C. Santa Cruz. I have

read the Draft Environmental Statement and all of the reports that analyze the earthquake hazards to the Lab, and that's what I'm going to focus on.

I agree with the Department of Energy that the two most critical issues are the potential ground motion at the site at Livermore and the possibility of surface rupture beneath buildings at the Lab. And as I said, I've read all the reports concerning this. I'd like to briefly review what those reports said about ground motion at the site.

Originally many of the buildings were begun to be built out here in the 1950's and '60's, beginning in '52. In 1971 there was an earthquake in Los Angeles, the San Fernando earthquake of 6.6 magnitude that caused exceedingly high ground motion. And because of this the Atomic Energy Commission ordered the Livermore Lab to do a study of the earthquake hazards at Livermore. This was the first one that was done, and it was published in 1972.

In this report, which is a Lab report, there is a map, which I have copies of here, and I have the report here. This map shows two earthquake faults going through the property beneath buildings on the Lawrence Livermore Lab site, the Tesla fault and the Coral Hollow fault. It has been determined that the Tesla fault is the major active fault, is connected to many other faults, and it has the potential for surface rupture beneath the buildings.

Now, in this report that was published in 1972, the geologist who did the report estimated that the ground motion from an earthquake on the Tesla fault culd be .8 g. And they said that other local faults around the site could cause a .8 g. For those not familiar with g values, that's how they measure ground motions. One g is approximately the force of gravity.

Now, later that year, the Lab hired another consultant, John Bloom, to do another report. And he came up with a figure of .5 g. Well then, in the next couple of years there was another study done which was the report that is the Appendix 2A in the Environmental Impact Statement. This was done by Mr. L. White, and it's a seismological and geological investigation of the Livermore site.

Now, in this report which is in the DEIS, he shows 12 active earthquake faults that could cause ground motion at the Livermore site. Now, three of these are large faults that are a ways away -- the San Andreas, Hayward and Calaveras faults. But he says that they could cause ground motions at the site of up to .5 g from the San Andreas and Calaveras faults.

The Calaveras is ten miles from here and in the Livermore Valley itself there are ten active faults. One wasn't discovered until later, although it was indicated on these maps. In this DEIS it has the maps of nine of these

faults, including the ones that go directly beneath the Lab.

Again, in the 1974 report, which is in the DEIS, the investigators concluded that you could have a .8 g ground motion at the site. And in the conclusion of that Appendix 2A he says that the Livermore Lab management decided that they wanted to go with the .5 g as a safe shutdown earthquake in the design basis for all the buildings out here, in spite of the fact that two out of three of the investigations that were done in the early '70's showed that there should be a .8 g plan. The Lab management chose the .5 g, and now when you talk to the people out here, you talk to the PR guys and the structural people, and in all their reports they say that they are going with the .5 g, that they have undertaken structural modifications to bring the buildings up to .5 g,,and that Building 331, which is the plutonium metallurgy building has been modified to withstand a .5 g, and that the glove boxes and filter systems and other things that were discussed earlier have been tied down for .5 g. And yet two out of three reports for the Lab say it should be a .8 g.

Now meanwhile, a controversy arose at the Vallecitos Nuclear Center near here, and the NRC shut it down, the reactor there. And they have determined that at Vallecitos, which is about eight miles from here, you could have a 1.0 g

or in excess of a 1.0 g. And there are indications from several studies that at the Livermore site you could have a 1.0 g. In fact this afternoon Friends of the Earth will be presenting testimony from seismologists and geologists that show that you could have as high as a 2.0 g and possibly up o a 3.0 g, because you have the Tesla fault going right through the site beneath buildings. And when you have a fult beneath a building and you have surface rupture on the fault, it causes enormous ground motions in excess of gravity, and just rips a building to pieces.

Now, the issue that I'd like to raise is, what if you have an earthquake on the Tesla fault and it damages the buildings where the plutonium is? Now, this morning I heard this man over hear say that there were just a few kilograms being dealt with. And yet in a document from the Labs it says that there are 500 pounds of plutonium in Building 331, the plutonium metallurgy building, and that at any particular time approximately 100 pounds of that plutonium is in process, is being powdered and processed into a form that can be used in what's going on here.

So you've got 100 pounds of plutonium in active process in the buildings available for dispersion, you have 500 pounds of plutonium in that one building, and the Tesla fault goes right by that building. And what I think the Department of Energy needs to do is to hire some structural

engineers and some geologists and some seismologists who are not Lab personnel to do an independent study of this, not only to study the structural engineering and the geology, but what would be the consequences if that plutonium is dispersed.

Now, it says in these documents that the plutonium can be dispersed as an aerosol powder which is dangerous, because if you breathe in plutonium aerosol powder it can cause lung cancer. And here we have an enormous quantity of plutonium that could give lung cancer to everyone in the Bay Area if it's dispersed during an earthquake.

The Tesla fault, by the way, had an earthquake in 1977, on June 21st, 1977. And it was a moderate earthquake, but it shows that the Tesla fault is active, and the maps show the Tesla fault going right through the property beneath the building. And I think that this is the most crucial and urgent issue that has to be dealt with. And as a result, several organizations, including Survival Without Nukes, Friends of the Earth and Congressmen Dellums are requesting that the Department of Energy remove all of the Plutonium and other radioactive materials from the Lawrence Livermore Lab, because of these 13 active faults, and because of the seismic activity here. This is one of the most seismically active areas in the whole world, and especially in the United States.

MR. FARMAKIDES: Thank you, sir. I think that is one of the issues, obviously, that this Board is going to recommend be considered seriously by the decisionmakers. And we are going to focus on this issue and articulate it.

Did you have a question?

DR. GROSE: I have one question, Mr. Barlow. You mentioned at the beginning of your presentation a paper published in '71 or '72. Do you recall who the author is, where that paper appeared?

MR. BARLOW: Yes, I do. It's by Mr. Tocarts and Mr. Bruenreuter, and they are both Lab personnel who are still with the Lab. I have a copy of it right here. The number is UCRL 51193, May,1972.

DR. GROSE: That came out prior to the first Bloom report?

MR. BARLOW: That's correct. Here is the first Bloom report dated December, 1972, which tried to downplay the hazards which the management later chose. But it's a much less conservative estimate of the ground motions. And as far as we're concerned, it's a cover-up.

MR. FARMAKIDES: Thank you. Did you have any other questions, sir?

MR. BARLOW: Yes. I have a question about the evacuation plans. In light of the recent Three Mile Island accident, I wonder what are the contingency plans for

evacuations in case of an earthquake that damages the Livermore Lab and releases plutonium and radioactive iodine gasses and other materials into the environment? If the wind is blowing towards San Francisco, Oakland, Palo Alto or San Jose, are there plans to evacuate those areas which are so close to here?

According to the Federal Government, you have to have plans for a nuclear power plant to evacuate people within 25 to 50 miles. Now, within 25 to 50 miles of here, there are approximately 25 million people. I want to know if the evacuation plans are available to those people, if those people know that they might have to be evacuated; and as far as I can tell from the DEIS, the only evacuation plans are for the people that work at this site. And the city of Livermore has some contingency plans. But beyond the city of Livermore there appear to be no plans. And we'd like to know when these plans are going to be devised, when will they be rehearsed, and when will the people be notified in the Bay Area that they may have to evacuate after an earthquake at Livermore?

MR. FARMAKIDES: Mr. DuVal, do you have a response to that, sir?

MR. DU VAL: Yes, Mr. Chairman. The DEIS analyzes the maximum credible accidents for a number of situations and concludes that there is no circumstance with release

of materials at Livermore that will create a need for evacuation beyond the site boundaries.

MR. BARLOW: The DEIS does not consider earthquake damage to the buildings at all in any of the accident scenarios. That is one of the number one criticisms of this document, that it does not consider earthquake damage to the Lab.

MR. FARMAKIDES: Can you respond to that, please, Mr. DuVal?

MR. DU VAL: Yes. The materials that are used -- the quantity of material that is used the calculation of the maximum credible accident are the materials that would be in work in process stations in the buildings which, in the event of fire or any other disruption, could escape from the building and could therefore pose a threat and which was analyzed.

The question of how that material escapes in terms of magnitude of the earthquake, we feel we're within the range of .5 g that has been discussed earlier in the report that you cited and that an earthquake, therefore, the hazards that you're talking about will not create an additional situation that's beyond what has been analyzed in the maximum credible accident.

MR. BARLOW: I understand, Mr. DuVal. But one point is that in your accident scenarios from fires and et

cetera, you assume that the filter systems are going to still work and that the filters will filter out 99.9 percent of the plutonium and other radioactive materials. My question is, what if an earthquake damages the filter system and there's a hole in the wall, and all of the radioactive gasses go out with no filter systems operating? You did not consider that accident scenario.

MR. FARMAKIDES: Did you have anything further, Mr. DuVal?

MR. DU VAL: The accident scenario you're describing is for a lateral ground motion greatly in excess -- or in excess of the .5 g which is the level to which the buildings are capable of withstanding.

MR. FARMAKIDES: I think it seems now clear as we're moving along that that will be one of the issues that we will pinpoint in our report, and that is the point that you're making that there are some calculations that bring this up to a .8 and others bring it up to a .5. We'll focus on that and raise that as one of the issues.

What the final decision is, that I do not know. But we'll include that in our report as one of the issues that has to be considered.

MR. BARLOW: Can you request that more adequate accident scenarios be done to consider earthquake damage to the Labs in which filter systems are damaged?

MR. FARMAKIDES: We will also include that issue. Yes.

MR. BARLOW: And evacuation plans?

DR. BEARD: Mr. Barlow, you identified plutonium and you also identified radioactive iodine. Will you identify the source to us, or where does it come from?

MR. BARLOW: Yes. In the DEIS it says that approximately -- I think it's 1,500 curies of radioactive iodine gas would be available in one of the accident scenarios.

DR. BEARD: Would you identify the source? Is it in the reactor?

MR. BARLOW: I do not know. The discussion is in the DEIS about the iodine.

MR. FARMAKIDES: Could you give us the page or the paragraph or some reference?

MR. BARLOW: Well, it's under the section called "Accident Scenarios."

MR. FARMAKIDES: All right. Thank you, Mr. Barlow. It's 11:20. Let's recess for about a ten-minute water break. And we'll reconvene in ten minutes.

(Off the record.)

MR. FARMAKIDES: May we proceed, please. We're going to take some very minor liberties with the schedule here. I have page 2 here apparently that I had started with. And I should have started with page -- there were no numbers on the page. There were three people that wanted to talk soon, because they have other commitments. And I hope that the rest of you won't mind. Those people did request an early morning time, and I just didn't realize it. The three people are, in this order: Dr. Johnson, Dr. Riggin, Dr. Schwartz. So we'll take those three people in that sequence. Dr. Johnson?

STATEMENT BY DR. CARL JOHNSON
DENVER, COLORADO

DR. JOHNSON: Mr. Chairman, my name is Dr. Carl Johnson. I'm a resident of 42 Hillside Drive, Denver, Colorado. I'm here as a private citizen and also as a person with training in public health, to give some input into this from the public health viewpoint and also from the viewpoint of a physician.

In reference to the Environmental Impact Statement, I note that about 7,000 employees work at the plant. I would like to see a discussion in the EIS of epidemiological investigation of possible health effects on these employees and the ones who have worked here in the past.

In Section 1, page 4, in 1976 about 10 curies of

tritium were released into the sewer system of Livermore, and 150 microcuries of plutonium 239 were also released to the sewer. In the EIS I would like to see to which streams the treated sewage was discharged and whether any communities downstream are using the water as part of their water supply. I'd also like to see tests of finished water in communities for plutonium and americium.

Section 1, page 4: In 1976, 4,000 curies of tritium and 470 curies of argon 41 were released. I would like to know from the EIS if any radioactive particulates were released as well.

On page 5 of this section, during 1976 the estimated maximum annual fence-line doses from Building 31 from radioactive gasses, argon 41 and nitrogen 13, oxygen 15 and tritium totaled about 10 millirems. I'm wondering if there are any other radionuclides which may have contributed to exposures in addition to these three gasses.

Section 1, page 6: Have any releases of toxic chemicals occurred to the sanitary sewer from the shop areas?

Section 2, page 9: What is the earliest date for release of radionuclides to the environment from the plant and what were those releases? I would like to see some reference and also some tables summarizing the airborne releases of radioactive particulates in gasses from the inception of the plant back in 1952.

In Section 2, page 16 I note that the transformer accelerator may cause above background neutron and gamma exposure rates at the south boundary fence. In reference to the neutron emissions it seems to me that there have been some recent reevaluations of the effect of neutrons, the biological effect, which could be discussed in the EIS -- the fact that this is still unresolved, the exact biological effect of neutrons.

Building 231, again in the same section -- I note that in Building 231 that plutonium, uranium and other isotopes are stored in that building which has a once-through ventilation system protected by only two HEPA fileters in series. At the Rocky Flats plant, which is in my county, they have five and six filters to protect such ventilation systems, rather than merely two. The records of the plant there show that with individual filters you may have warpage of the filter, filters have been installed backwards, which essentially reduces the effectiveness of the filter. So two doesn't provide very much assurance against such unplanned releases.

In building 251, diagnostic chemistry, kilocurie quantities of actinides are stored here. These include, I presume, radium, plutonium and perhaps curium. I don't know. I'm not sure. These actinides are handled and processed here in specialized enclosures. Does this building release any radionuclides to the air from ventilation to the exhaust stacks or in the sewage waste?

In Building 281, which contains the pool-type reactor, this building is noted to release argon 41. This release was reduced in 1973. In 1976 470 curies of Argon 41 were released compared to 4,000 curies of this gas in 1972. I would like to know if from the reactor core, if any other radioactive gasses are released, or radioactive particulates, such as are released by other reactors, including neptunium 239 and others, as well as fission products.

I note that the 1976 argon 41 release corresponds to an estimated site parameter radiation dose of less than 1 percent of the accepted standard. This implies, if I read this correctly, that four years earlier a radiation dose of about 10 percent of the accepted standard might have been sustained at the perimeter. This becomes of interest because, as you know, the current radiation protection standards are under review, I think, with consideration for reviewing or reestablishing these protection standards at lower levels. So this could well be significant.

I would also like to know if larger amounts have been released in the past, over the past 27 years.

The EIS I think should have a paragraph showing the maximum possible total dose to persons from all radionuclides, both gasseous and particulate, including estimates of effects from internal emitters, like plutonium, which tends to remain in the body with a very low rate of turnover -- some say with

a turnover of about 200 years.

I would like to see estimates for such doses to populations at 10 kilometers, 20 kilometers and 30 kilometers, with special attention to internal doses from interal emitters which may have been ingested or inhaled, particularly those that may have been inhaled.

In Building 331, which deals with gaseous chemistry -- this building does experimental work with radioactive gases and their compounds. The primary gas is tritium and multi-kilo curie quantities of tritium are used in a variety of forms. The EIS states that the current annual site boundary radio dose from the plant is estimated to be less than two millirems. But I wonder if other radionuclides may be released also. There is quite a list of various volatile and other chemicals which may vaporize and escape. The building is doing research in the area of chemistry. I would wonder about such substances which could go through a filter because they are vaporous.

Building 331, metallurgical chemistry: In this building research work in plutonium is carried out, including development of plutonium fabrication techniques, as well as basic and applied metallurgy. Does this include milling of plutonium metal? If so, during operation what quantity of plutonium is drawn up in the exhaust air through the filters? In the plant in my county, I have been told about 180 grams

goes up the stacks each day. And some significant amounts of that do pass through the filters into the exhaust plumes from the plant. What are the annual releases to the atmosphere from this building of particulate plutonium, amerecium and curium? And how much has been released year by year in the past since 1952?

In Building 514, liquid waste treatment plant: Have any radioactive wastes been released to the sewer by accident from this building?

Building 612, solid waste disposal: I would like to see if there have been tests monitoring releases of plutonium and other radionuclides from that building back to 1952. In the SLL tritium research laboratory, tritium research laboratory, experiments done there involve kilocurie quantities of tritium, and ventilation there is provided by a once-through system. I am wondering there about the amounts of tritium released to the atmosphere from this building over the years.

Secton 2, page 27: Environmental monitoring. I would like to see copies of environmental monitoring reports from the plant back to 1952 for the particulate and gaseous effluence in the air, as well as liquid effluence from the plant.

Section 2, page 63: I note that 17 samples taken in the drainage ditch along the waste disposal area

had an average of 887 picocuries of plutonium and 153 picocuries of americium 241 per gram of dry soil. I don't see in the EIS whether this drainage ditch drains. It doesn't appear to drain anywhere from the sketch. If it does drain, have samples been taken downstream? And have they been analyzed for plutonium and americium? And in addition, I would like to see more survey data of the possible effects on water in the underground water stream under the plant.

I would like to see a great deal more soil sampling done, up to 20 and 30 miles from the plant. This has been done in the Rocky Flats area to a distance of 32 kilometers, some 20 miles. In fact, I think that the environmental measurements laboratory, has done tests at even greater distances. And from these they have drawn isopleth lines or contour lines showing lines of equal contamination of agricultural soil. These are very useful, in that in order to double check your risk estimates for a population you can conduct detailed cancer incidents investigations to see what actually has occurred in the population in the vicinity of this plant, as well as others like it.

I would like to see those samples, besides including core samples which address inventory amounts of radionuclides, to also include surface dust samples, because the risk from the internal alpha emitters like plutonium and amoricium is primarily from inhalation and to a lesser degree from

ingestion of water and food. And so the surface dust then becomes more of a possible threat to persons living in the area, as well as indicating the more recent patterns of contamination in plumes from the plant. Even a low level of plutonium in surface dust indicates that it's been soiled by plumes from the plant. At greater distances this effect is more important, because the larger particles of these nuclides settle out near the plant. But the particiles smaller than one-tenth micron behave like gas molecules and don't really settle out to any great extent, but blow for many miles.

On page 12 of Section 3 I note that radioactive waste is transferred to a DOE site for long-term storage if above ten millicuries per gram. If below that level, the waste is transferred to a commercial burial area in Nevada. I'm wondering about any spills of radioactive materials in transit over the past 25 years.

On the same page I note that draining from the building goes to retention tanks outside the building. How large are these retention tanks? And I would like to know if any fission incidents have occurred. The reason I mention that is that water suspensions of plutonium oxide can undergo spontaneous fission in liquid form and boil and bubble for up to three days, releasing large amounts of fission products which might go undetected if you aren't looking for them.

On page 14 of Section 3 I note that a stack monitor

has been required for the reactor. I would like to see copies of the monitoring data from this monitor over the past 25 years for this building and for the other buildings which have such monitors, especially for building 251, where because of perhaps milling operations being conducted, alpha contaminated particles may have escaped into the atmosphere.

Page 20, Section 3: I note that the minimum detectable concentration of plutonium 239 here is 30,000 picocuries per liter in the domestic sewer effluent as it leaves the site the southwest boundary. This is a very heavy concentration of plutonium in terms of implications for health effects. I'm wondering how often the sewage has exceeded that figure over the past 25 years.

On page 21 of Section 3 I note that releases of radioactivity to a sanitary sewer system have been usually more than three orders of magnitude below permissible levels. But this is still of some concern because a recent report in "Science" indicates that the uptake of plutonium from the intestinal track is enhanced by 1,570 times when the water with the plutonium has been chlorinated, as does happen in water districts.

Table 1 in Section 3 indicates some rather large releases of radioactive materials in 1976, both in air and in sewage. The nitrogen 13 releases and oxygen 15 releases would appear to be of special concern because they may enter the

food chain. I note that 170 microcuries of plutonium, a very large amount, was released in sewage that year, as well as 3,200 microcuries, a very large amount, of unidentified alpha emitters. I believe that that much radioactive material should have been identified, and certainly monitored and prevented from being released from the plant site. This could have contained americium and curium.

On page 27 of Section 3 I note that treated sewage from the City of Livermore is used for irrigation of agricultural land. Have tests been performed of plutonium contamination of surface dust on this land, dust which may blow about and be inhaled? And also, have tests been done on produce grown in this soil?

I olso note that sludge from settling tanks are used for soil conditioning and for gardening, which could result in some exposures due to inhalation of resuspended dust with plutonium and uptake by produce of plutonium. Recent studies of plutonium particles in soil done at Rocky Flats indicate that these particles are very small, many smaller than one micron. They could easily blow for miles downwind after being resuspended from such areas where this material has been used.

The particles which I keep mentioning are important because some experts in the field with conservative views, like Carl Morgan, have risk estimates which suggests that two or

three particles of plutonium of respirable size, or americium, or perhaps even one of curium, would exceed the permitted doses -- that is, doses he would recommend for the general public, and which would produce some risk of excess cancer rates or birth defects.

Now, I know that in the response to a suggestion that respirable surface dust soil be used for evaluating off-site contamination, that there were some objections to this from Department of Energy personnel. These arguments are very familiar to me. They are addressed in my article in "Science" of August 6th, 1976 and also published again in "Science" in the following year, and I think handled very adequately. The samples I have used are treated by methods developed by persons with doctorates in soil science to separate the respirable fraction. Agricultural samples used by Department of Energy which include coarse particles two millimeters and smaller are ground to a very fine dust. From there on, they are handled very similarly.

The difference is that you find much larger amounts in surface dust. In the study I did, I found as much as 285 times more plutonium in the same location as had been found with agricultural soil samples. And in looking at estimates of risk to health of persons who may inhale dust from such areas, surface dust provides better information on such risks as does agricultural soil.

Page 33 of Section 3: I would like to know if detonation of test assemblies can suspend micron sized particles of uranium, berylium, thorium and perhaps plutonium in the air so that they may present a respiration hazard to persons in plums downwind from the test sites? We would also like to know in the EIS if these tests are similar to those done in Nevada to which some attribute soil contamination of plutonium at great distances from the test sites, due to blasts with conventional explosives, testing distribution of these radionuclides.

I also see in the report that 50 percent is estimated to have been deposited within the site boundaries away from the shot location. But surely much of the remaining 50 percent must be blowing off site as well, which means tha during parts of the year, perhaps 20 percent ormore, it's blowing towards heavily populated areas. Apparently tritium has also been released by the shots.

Page 36, section 3: I see there are annual reports documenting the measurements of radioactive materials in air, water, soil and vegetation at various points in Livermore Valley. I would like to have a copy of each of those reports back to 1952. And I would also suggest that when these values are reported, if you're looking at potential health effects, that they be reported in meaningful units, like picocuries per cubic meter of air, or pecocuries or pintocuries per liter

of water, so that they are more easily comparable from area to area. In the case of soils, I think that to take a sample five centimeters deep and give the results in an awkward method, an awkward unit in terms of grams per soil is really unuseable in terms of looking at risks from surface dust being suspended and inhaled.

Page 37, section 3: It's noted that in 1976 all levels of radioactivity observed were well below the appropriate concentration guides. These guides, especially for plutonium and americium, may be revised downward by a factor of two to three orders. On this page, the total annual dose to the nearest resident is estimated to be less than two millirems per year, compared with 100 millirems per year from natural sources. One-fourth of that was from argon 41.

What I'm asking is whether this does consider inhalation plutonium and other transuranium nuclides which may be inhaled and set up essentially a lifelong residence in the bod.

Page 52 of Section 3: I note that some 60 accidents have occurred since the beginning of the nuclear industry, most of which were due to human error. And a number of these did occur here at the plant, which means that we must take precautions to prevent exposures of large populations to releases of radiation, especially particulates, in my opinion, from such accidents.

On page 53 of Section 3 there is a table showing available isotopes from ten to the 18th fission accident. I would also like to see a complete list of particulate trans-uranium nuclide releases as well. I don't believe all of those are registered on this table.

Page 36, Section 3: I note that building filters in building 251 will pass, at most 40 percent of the airborne curium oxide to the outside atmosphere. One particle of curium oxide, about two microns, would exceed allowable doses, according to some experts in the field.

And 2C-1: Groundwater hydrology. I am wondering here about tests being conducted at distances from the plant for plutonium in finished water.

Appendix 3C: A review of accidents at Livermore. I am wondering how much curium was released by the fire which destroyed the contents of a curium processing cave in 1960. A few microcuries of curium is really a large amount in terms of potential health effects.

I am also wondering about what sorts of values were found for radionuclides released as a result of the nuclear excursion in Building 261 in 1963. I think it's quite likely that radioactive particulates would have been released as well as fission products. And these particulates are more important, in my opinion, than the fission products.

The release of 350,000 curies of tritium from

building 331 into the atmosphere, I think, is of some concern. I would like to see more work done with tritium levels in water downstream expressed in terms of picocuries per liter, as the EPA does at the present.

I think my main concern is the releases over many years of small amounts of radioactive particulates and plumes from the plant because of a concern with a similar plant in Jefferson County. I had done a brief evaluation, which I think should give some guidelines for studies to be done here. There we have documented releases of plutonium since 1953. In 1957 there was a fire which destroyed the complete filter system from glove boxes up through the main stack. According to one report, the top of the stack nearly melted in the heat. The fire burned for about 12 hours, and lots of black smoke poured out during this time.

But we have no records of releases during the fire. Before the fire, as much as 100 grams of plutonium oxide were found on single filters at the plant. Some 600 filters were destroyed in the fire, I understand.

We have no records for a week after, but on the eighth day following the fire, in one day the permissible limits for plutonium for such releases was exceeded by about 19,000 times, according to one estimate of such stack releases. It was equivalent to over 50 years' releases in a single day. And I am hoping that communication has improved, because at that

time an AEC spokesman said that there was no spread of radioactive contamination of any consequence, when in fact the entire Denver area should have been evacuated.

In the Rocky Flats area, we were ranking there on private lands around the plant between the Bikini Atoll and the Los Alamos Scientific Laboratory in the amount of plutonium contamination in the soil. And if we had a viewgraph projector here I would show you plume patterns from such installations. There are six of them. Four of them show plums along the ground, which create ground contamination according to the particle size and the length of contact of the plume with the ground. Such patterns are quite comment.

In the case of very fine particulates from plutonium plants including research installations a report by Etinger and Gonzales shows that such particles are in the micron and submicron range and are not visible. These plumes cannot be seen because they are alpha emitters not easily evaluated by detectors as well. You can get some idea by setting up air monitoring stations. But the filters in those air monitoring stations are not very efficient, according to tests done at Rocky Flats.

A chart shows that these particles smaller than one-tenth micron move like gas molecules and that the Colorado State University studies indicate that plutonium oxide on such filters over long periods tend to move through filters

by diffusion, and so you find millions of particles downstream in the plumes from the plants, despite claims from manufacturers that these filters are 99.97 percent efficient.

In the Nuclear Air Cleaning Handbook, which was reviewed in Health Physics in February of last year, it was pointed out that the photoelectric methods used to measure such filter efficiencies are very misleading and should not be taken as a true indication of the true efficiency of these filters.

The fission incident I cited as possibly occurring in water suspensions and collecting tanks underground is cited in the Rocky Flats Environmental Statement of 1975. They also cite a second one that could occur due to incorrect stacking of metal ingots of plutonium. They also cite instances in which -- when over 1,000 barrels of lathe oil tainted with uranium were burned in an open ditch. I wonder if there have been similar burnings at the site here. If so, they should be listed in the Environmental Impact Statement. Also, if there have been burials elsewhere in this area offsite of the current boundaries of the plant, those should also be listed.

I would hope to see in this area, and perhaps as part of the final EIS, a survey done by the Environmental Measurements Laboratory out to 20 miles around the plant and developing isopleths from which epidemiologists can conduct epidemiological investigations of cancer rates over the past ten years.

I'm also hoping that they will use, in addition to inventory samples of the cores, also samples of surface respirable dust in order to get better indications of what actually is presenting a risk to persons living in the area. Studies I have done around Rocky Flats to 20 miles, I found levels in private lands of plutonium as high as 3,390 times that from worldwide fallout in what would have been somebody's back yard in land approved by the State Health Department, with concurrence of experts at Rocky Flats for residential development.

In my opinion, such areas where you may have children playing for hours -- well, days, actually, in the surface of the soil, we whould consider a plutonium concentration limit similar to that used by the Russians. They have one standard which is 100 times less than that proposed by Dr. Burleigh of the EPA, which applies to laboratory workers for work services.

There is another one in use in the United States for truckers with the Interstate Commerce Commission, which permits only one-tenth of that permitted by Dr. Burleigh of the EPA for residential areas. I think a similar level is also used by the Navy. So I would advise you not to use Dr. Burleigh's recommended level, but to look at the Russian standard, which seems to be more conservative.

I think that residential areas are different from agricultural areas because of the reason you may have children

playing on the surface of the soil. I think this makes it of some concern. I think the soil should be treated as a surface, like the surface of a truck or a laboratory surface.

In addition to surveys of cancer rates, I think it would be prudent to do a review of rates of congenital malformations over the last ten years. I have begun such a study in the Denver area and found higher rates near the plant. In talking with genetecists they tell me that their concern is not excess rates of cancer from longterm exposure from interal alpha emitters, but the longterm genetic effects which are not fully appreciated in the first generation, but tend to accumulate in greater efficiency in subsequent generations.

DR. BEARD: Are you still talking about plutonium?

DR. JOHNSON: Plutonium primarily. I'm also concerned about americium and curium. I think I've about used up my time.

MR. FARMAKIDES: Thank you, Dr. Johnson. Did you have any other questions?

DR. BEARD: Yes. Dr. Johnson, I respect everything you did. You've snowed me, frankly, and I've been trying to sit here mentally -- I divided the things, and you want a lot of information. And I didn't get clear what you wanted in an expanded Environmental Statement. I've made a lot of notes. And then you said that you would like a lot of personal information, as I gathered, and I take it that you didn't want that.

Then you put into the record a lot of your experiences and so forth, which we have in the record and can study about the health effects. But I would like -- not now, because the time is limited. If possible, I would like to really have you think through this and give us a list of what you want in the Environmental Impact Statement, realizing they can't go -- and what type of data you would like personally from the Environmental reports. I would like to see that. I'm confused.

MR. FARMAKIDES: Dr. Johnson, I would support Dr. Beard on this. What I would suggest -- perhaps the material that you were reading from you would give to the staff and have tnem review it, then, and I would ask the staff, you would, please, to respond to that -- to questions raised, because they're so numerous and they're so voluminous that very frankly, to do so today would be almost an impossibility.

DR. JOHNSON: It was my understanding that the purpose in reviewing those items was one to see if there were any that they were willing to respond to at this time, but also to include this as part of the written response. I have a written list of things I would want to present. And I also would like to see in the EIS some reference to epidemiological studies of cancer rates. I would like to see my studies cited of the Denver area. I found in a study of over three years of over a million people a very excessive rate of cancer in large numbers of people.

MR. FARMAKIDES: Could we have a copy of that, sir?

DR. JOHNSON: Yes.

MR. FARMAKIDES: We will take it in. And how do I identify it? Do you have a caption on the top?

DR. JOHNSON: It has my name on the caption, yes. There are other reports I would like to attach as well.

MR. FARMAKIDES: We'll accept that, then, as an exhibit to the record. We'll give it to the staff.

DR. JOHNSON: I'l assemble this and give a copy to them and a copy to your recorder.

MR. FARMAKIDES: Okay. Thank you. Does the staff have any comments with respect to the questions raised by Dr. Johnson?

MR. DU VAL: We would be prepared to respond to the bulk of the questions, where at all possible, and/or to point out where some of those questions have already been addressed in the staff study or the DEIS. But where not, we will do our best to respond to those questions.

MR. FARMAKIDES: Dr. Johnson, you've obviously done a lot of work and a lot of time, and we appreciate very much your efforts. Did you have any other questions, sir?

DR. JOHNSON: The question that I have: Would this Board support the recommendation that the isopleths maps be developed and that an epidemiological study be done with cancer rates and rates of congenital malformations in this area and

that data be considered over the whole 20-year period? It's certainly relevant to the environment.

MR. FARMAKIDES: Well, thank you, sir. What we will do is to take your opinions, your comments, and listen to all the rest here and evaluate them and come up with our report. At that time, we'll make a decision one way or antoher.

DR. JOHNSON: Thank you.

MR. FARMAKIDES: Thank you very much, sir. The next person -- again, we're taking people out of order, but the next person is Mr. Riggins, William Riggins.

STATEMENT OF WILL RIGGAN

MR. RIGGAN: My name is Will Riggan. I live at 2740 Hillegass Avenue in Berkeley. Before getting into my comments, which themselves I think will be very brief, I need some clarification on a point in this EIS process. I studied carefully the Department's response to our critique and noted that fewer than ten of the one hundred or so questions that we raised were addressed. What is particularly troubling is that we may be in considerable disagreement over what constitutes an adequate response by the Department. For example, we charged that the DEIS was deficient in its coverage of at least fourteen general issues. One, for example, was an inadequate demonstration of regard for the health and safety of LLL-Sandia employees. You responded - not you personally, but the Department responded - to that charge with a two-page litany on LLL general safety procedures, but you only addressed four of the seventeen specific questions that we raised that led us to conclude that the draft was inadequate on that subject. And we expect those questions - and our other questions - to be addressed specifically.

MR. FARMAKIDES: Are you asking for this Board

MR. RIGGAN: Yes.

MR. FARMAKIDES: Insofar as the staff can, I think that they will. Now insofar as this Board's authority to require the staff or anyone else to respond, I doubt that. We

are an ad hoc Board, we are brought together for purposes of this hearing. We're going to try our best to develop an adequate record. If the question is one where we - this Board - feels is critical to our understanding, we will get an answer. But that -

MR. RIGGAN: Perhaps then I should address this to Mr. Pennington to find out what DOE regulations require in this EIS process. That is, if it is not required for the Board -

MR. FARMAKIDES: Well, the thing is this, Mr. Riggan. I think in the final analysis you can't make the rules yourself.

MR. RIGGAN: No, I'm trying to get an interpretation of the rules, and I haven't gotten one so far.

MR. FARMAKIDES: Right. And your point of view is your own. We very much respect it. We want to hear it. But your point of view isn't necessarily the final point of view. There's got to be a balance here.

MR. RIGGAN: Oh, I'm not trying to argue for my point of view. What I'm trying to do is to get an interpretation of what the EIS process requires of the Department of Energy. We filed the critique; it was our understanding that all questions addressed to the Department would be answered in detail. That those that were considered relevant to the process itself - I understand that there is dispute over some of our questions -

MR. FARMAKIDES: Let me ask the staff as to their

interpretation.

MR. DU VAL: There was an effort to categorize the numbers of questions that were received into general categories and to try to deal with them in that grouping as much as possible. Where you feel, though, that we have not responded to that I can tell you we will go back and address the balance of those issues you feel have not been responded to.

MR. RIGGAN: Okay. Thank you, Mr. Du Val. I'll submit a letter to you?

MR. FARMAKIDES: Mr. Riggan, could you possibly talk directly to Mr. Du Val? Would you be the proper person?

MR. DU VAL: That would be fine. I'd be happy to talk to you, Mr. Riggan.

MR. FARMAKIDES: Could you submit -

MR. RIGGAN: All right. What I'll do is, I'll submit a letter to you with a copy to you, Mr. Farmakides.

MR. FARMAKIDES: By when, sir?

MR. RIGGAN: By, say, early next week I'll mail

MR. FARMAKIDES: Monday.

MR. RIGGAN: Monday.

MR. FARMAKIDES: The reason is this: we too have to submit our report. And we have a deadline on you people so that we can get it from you. Now what will happen if you postmark it Monday, as I've asked Mr. Ladd to do, then I should get it within three days. If you could kindly also concurrently with

sending it to me send a copy to Mr. Du Val it would help things.

MR. RIGGAN: All right. I certainly will.

MR. FARMAKIDES: Okay.

MR. RIGGAN: Thank you. There is a fundamental issue at stake I think in this environmental impact proceeding. It has lots of interpretations. Some of them are narrowly defined, some of them are defined as broadly as the survival of the species. Basically it is the health and safety of people. Some define it as the world, some define it as the Bay Area and some define it as the people who live here in the Valley; and some restrict it simply to employees of the Laboratory. A corollary issue in this matter and the one I want to focus on briefly in my remarks is the credibility of the Lab's managers and the credibility of the Department of Energy in this process. It seems to me that the burden of proof has shifted. At one time we were encouraged and most of us did in fact believe our experts in the nuclear industry. Now I think the burden is on you to demonstrate that what you say is true. That is, until evidence is offered, we should not believe it, simply for the sake of our own health and safety. For years there have been assurances from the Lab managers on the seismic safety of the Laboratory, and yet we find in the draft Environmental Impact Statement that there is a report from Dr. Bloom in 1972 that was reiterated by Mr. White in 1974 that said that the Tessler Fault first strand probably ran under the

Laboratory. That fault has not to this day been mapped.

For years there have been assurances from the Laboratory that they are concerned with the health and safety of the community. And yet now we have evidence from Dr. Johnson and others that things are perhaps not as they have seemed. I think that the latest example and clearly an outrageous statement was made by an LLL spokesperson the other day in response to the questions about the history of accidents. I quote from the Berkeley Gazette. "Some of the accidents cause the release of radiation into the environment, according to a government report, but LLL spokeswoman Linda Curry said Monday that the health of lab workers or the public was never in danger." That is an irresponsible statement. We cannot say that people definitely were in danger, but we equally cannot say that people were not and never in danger.

As a consequence, since the Department, I think, cannot be trusted without independent evidence that other people can look at and since the Lab cannot be trusted without independent evidence that others can look at and analyze, I would like to request two things on two of the, what seem to be the central issues as you define them; that is, one, the seismic question and secondly the health and safety of workers and people who live here in the Valley.

First I think when the seismic reevaluation is completed, there should be a public forum here in Livermore

sponsored, if not by the Lab managers, then by the Department of Energy that will be open to all people, that will proceed like a public hearing with invitations for experts from all sides and all persuasions to sit down and in a public forum evaluate the meaning of this seismic, of this upcoming new seismic report, before, before any moves are made to insert that report into the final Environmental Impact Statement.

Secondly, is a consequence of Dr. Johnson's questions until answers are provided there. I think that I would like to argue that a study of cancer rates and of congenital malformations here in the Valley to a distance of twenty miles from the plant is an essential part of this Impact Statement, and that no responsible Board, it seems to me in the wake of the questions that he has asked, can proceed to recommend a final Impact Statement before those cancer studies have been done. That this Board should so argue, and that after those cancer studies are done, and before they are made a part of the final Impact Statement, there be a similar public forum, a public hearing with health experts again of all persuasions allowed to address their understanding of the meaning of the that study.

Thank you.

MR. FARMAKIDES: Mr. Riggan, thank you very much, sir. Your comments are very thoughtful, very cogent. We will include in our report those two last suggestions that you make. I think it's something to be valuated, whether or not we can have

those, it's not my decision to make. But we will include those.

MR. RIGGAN: I understand. Thank you.

MR. FARMAKIDES: Thank you. I think next is Mr. - It's 12:25. Mr. Schwartz, do you want to go on now, or do you want to have lunch?

MR. SCHWARTZ: I'll leave that up to you, sir. If allowed to, I have a substantial body of material which will take well over an hour. We have already discussed that you may try to rule part of it out of order. I don't know how that's going to proceed.

MR. FARMAKIDES: Please let's sit down.

MR. SCHWARTZ: We could start now and then adjourn and resume after lunch -

MR. FARMAKIDES: I think we should start and perhaps discuss it on the record. You did come up to me during the break and I think you have been given thirty minutes. You now would like considerably more. My concern was after I read your letter, it seemed to me that it was completely irrelevant to the draft statement. And very frankly I asked you then during the break if you could take the same amount of time as I gave to Mr. Ellsberg this morning, because I did not consider your comments in your letter specific to this particular statement. But you said that you did have information that would in fact tie your comments to this statement, so sir, I

would like to hear that.

MR. SCHWARTZ: All right. I have put over there copies of my March 30 letter to Mr. Pennington which serves as a kind of outline for the topics I'd like to discuss. I believe you also have copies of it, but I also have here a packet -

MR. FARMAKIDES: Is that the same letter?

MR. SCHWARTZ: This packet which I'll give you is a collection of documents which I hope to submit as part of my testimony. Now, first I should introduce myself.

My name is Charles Schwartz, S-C-H-W-A-R-T-Z. I am a professor of physics at the University of California, Berkeley, and that can serve as my mailing address. I have been for many years an active member of an organization originally called Scientists and Engineers for Social and Political Action, more recently called Science for the People. I have also been actively involved with the UC Nuclear Weapons Labs Conversion Project. I am speaking here for myself.

I responded in a December 15th letter to the original Environmental Impact Statement draft, raising a particular issue, and in my followup letter March 30 I pointed out that the questions I had raised had not at all been responded to in the draft statement, that I wanted to appear here, that I wanted the Department of Energy, as I understand they're supposed to, a number of expert witnesses to answer a number of specific questions; the area of concern is the risks of

nuclear war as implied and as produced by the activities of the Livermore Laboratory. I asked for an hour and a half to make my own presentation and estimated if the witnesses requested were brought forward, they might well take another hour and a half - I think we're going to have to see how the time goes.

Several people have referred to the post-Three Mile Island atmosphere, and I think it is extremely relevant. I think the burden of proof has shifted from the government's posture of "trust us" to the people's new requirement of "first convince us it is safe or else stop it." I think the government has the absolute obligation to tell the people the whole truth about the hazards we are subject to, and that will be the truth about hazards implied in the nuclear weapons work of the laboratories which the staff has tried to define out of the Environmental Impact Statement. There are two parts to this - one part was what was discussed by Mr. Ellsberg and it also came up at the Rocky Flats hearing. This is the assertion - and it's very clearly and it is the prime assertion in the draft Environmental Impact Statement that it is the benefits to the national security that is the prime positive reason for having the laboratory there. And these other people have questioned the nature of that benefit. I am not going to deal with that aspect. I'm going to turn the coin over and say to compare with any benefit is a risk. And I want to ask the government to and I am going to present data to start the dis-

cussion of looking at and assessing the risk associated with the continued developments of nuclear war and the possibilities of provoking nuclear war and what would be the consequences of that.

MR. FARMAKIDES: Sir, I want to go back to our original discussion, and I think that that is out of order.

MR. SCHWARTZ: Well, I would now like to make arguments as to why it should be. I have three arguments to make -

MR. FARMAKIDES: I'll hear your arguments.

MR. SCHWARTZ: And I would like to be able to present those arguments in some substance.

MR. FARMAKIDES: I'll hear your arguments. I'll give you five minutes.

MR. SCHWARTZ: The first argument is the one I've just indicated. That it seems only logical if risks of a certain course of action - I'm sorry, if benefits are to be asserted, then the risks must also be evaluated. We have been told in the draft statement that the benefits of the national defense are the positive side; it then seems logical to look at the negative side as well.

The second argument is a legal or legalistic one which I can't evaluate it, but I found it quoted in the Rocky Flats hearing by Mr. Ruysman (?). He said in a case that the National Resources Defense Council brought against then Secretary of Interior Morton, it was held by the U.S. Court of

Appeal that under the National Environmental Policy Act the absence within an agency - and this would perhaps apply to DOE - the absence within an agency of the ability to implement a course of action does not excuse them from the obligation to explore the wisdom of the course of action, and that is what I'm requesting, an investigation of the wisdom of the course of action of nuclear weapons development.

The third argument ties most concretely as you have requested to the documents before us. I'm referring now to the staff response to the questions, and this goes on pages 2 and 3 in which the staff - and I understand this is the staff of Livermore Laboratories, isn't it? That wrote the staff response?

MR. FARMAKIDES: Yes.

MR. SCHWARTZ: In which they tried to define away questions about the mission of the Laboratories, about the nuclear weapons defense program as being not part of what this environmental impact statement is about. They just define it away without any further basis, except on page 3 they give a particular rationale, and I will demonstrate to you that that rationale is false. The rationale goes as follows: starting at the top of page 2 - "U.S. Defense Policy and Nuclear Weapons Requirements - In support of that policy, restrict alternatives as to DOE's weapons development activities." That means the orders come from Washington. This says, however, the converse is not true. DOE's development of nuclear weapons does not

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foreclose options with respect to the overall U.S. national defense program. I claim that that is false, and I would like to present detailed evidence indicating and showing that the relation between the Laboratory and government policy is not one way in which national policy is set and the Laboratory follows orders. That in fact to a very important degree, things flow in just the opposite direction. That activities of the Laboratory are prime movers in shaping weapons development which then become national policy. In that sense the activities of the Laboratory are to a large degree the primary place to look for questions about what are the results of the activities that go on there and cannot be sluffed off onto the convenient phrase of national policy. And I would like to present a body of information to support my claim that the staff report is quite false in its characterization of the direction in which orders flow.

MR. FARMAKIDES: Sir, I think we'll go back - perhaps you and I can discuss it during the lunch hour, but it seems to be very obvious that there is no way we can get involved in the breadth of the subject that you want to pose. We're here to consider a draft Environmental Impact Statement. We're here to evaluate here. We're here to discuss whatever issues that you have with it. What you're suggesting that we do is go way beyond the perameters of that particular mission and go into a national debate or a debate if you will of national policy

with respect to nuclear weapons. That's beyond the purview of this particular forum. Mr. Schwartz, I'm going to recess -

MR. SCHWARTZ: I'm sorry, sir. I would like to fundamentally disagree with your characterization of my remarks.

MR. FARMAKIDES: Off the record.

(Off the record)

MR. FARMAKIDES: Back on the record.

MR. SCHWARTZ: This is supposed to be a public hearing. I would like to know that public that's not here that might perhaps read these transcripts to know what we're arguing about.

The following is an outline of the presentation which I wish to make at this hearing. A - Argument as to why assessment of the risks of nuclear war must be included in the EIS. This was the central point in my letter of December 15, 1978 criticizing the draft EIS. The DOE staff statement in response to comments received dated March, 1979 was totally inadequate in responding to this issue. That was point A of what I wanted to do, and I started to do that - you haven't let me get very far. Part B - Search for answers to the questions posed in my December 15 letter, as follows: Number 1, what is the probability of nuclear wars of various sizes occurring? 2, what are the likely adverse consequences of such wars to the population generally and to the LLL area in particular. 3, How do the above-mentioned risks compare to other types of nuclear accident risks that have been much debated, from nuclear power plant

accidents, from earthquakes, from sabotage and terrorism, etc. C - Followup inquiries from A and B and other related matters in the DEIS.

MR. FARMAKIDES: Sir, I think you made the point. And that's why I think it's way beyond the scope of the draft of the statement. And that's the reason why you're out of order and we're not going to hear it -

MR. SCHWARTZ: Sir, you keep saying

MR. FARMAKIDES: We're off the record.

(Off the record)

AFTERNOON SESSION

MR. FARMAKIDES: May we proceed please.

During the recess the Board has talked to Dr. Schwartz and he has talked to us, and the Board has allowed between 20 and 30 minutes, up to 30, in which period of time Dr. Schwartz will present his views and relate them to the draft environmental impact statement.

Dr. Schwartz?

STATEMENT OF DR. CHARLES SCHWARTZ

DR. SCHWARTZ: Thank you, Mr. Chairman.

The general framework in which I'm relating my remarks to the draft environmental impact statement is I think that there are a body of risks that have not been acknowledged, considered in that report, that needs very much to be, first of all, pointed to and studied and evaluated. I would like to do some of that today in two forms, giving examples of particular risks, giving you some of my own estimates of the magnitude, asking questions for government authoritative responses to those. And, also, because I think it is relevant, trying to tie the source of the risk to the work of the Laboratories themselves, because I think that is the important link. Some of the risks impinge directly on the Laboratory, some are also produced by it.

Let me give the one example, which I mentioned to you privately before, which is very striking and ties precisely to

one subject that has been discussed here much already, namely, what is the risk from the large amount of plutonium, perhaps, several hundred pounds of plutonium that is routinely kept in storage at the laboratory? We are told that it is in large metal safes inside large concrete buildings. And the argument so far has been whether that is safe from some major earthquake.

I would like to point out another source of event, an unpleasant event to be sure, and I will argue that, in fact, not at all probably unlikely event. Consider the explosion of a hydrogen bomb at the site of the Lab presumably delivered from an enemy country usually considered the Soviet Union. If such an event happens, I put it to you, and I would like experts at the lab to explore this more quantitatively since they are more expert. If a hydrogen bomb were exploded somewhere near that building where the plutonium is stored, I believe the concrete would be vaporized, the steel containments would be vaporized, and the plutonium would be vaporized and spread all about. As we heard, the most nastiest scenario for plutonium can do to people if it is vaporized, put into finally particulate form and dispersed over a large area has the potentiality for creating enormous numbers of cancers in years ahead. I've heard people talk about one gram dispersed. Well, I have now given you a scenario by which several hundred pounds of plutonium might be dispersed.

And the question. Is that a plausible scenario, or is

it just some nut's fantasy.

Well, I put it to you that is in fact one of the most likely nuclear events to occur, and that's based on the current United States weapons policies which are in the direction of counter-force weaponry and counter-force policy since 1974 under the advocacy of then Secretary of Defense and now Secretary of Energy, Mr. Schlesinger, this country's military, strategic policy has been developed on the assumption that the primary threat was a counter-force strike from the Soviet Union. I don't know how ultimately rational it is, but this is considered the most seriously considered scenario by our national weapons policy makers. The idea is that the Soviet Union might launch a limited nuclear strike against some of our forces without killing too many people in expectation that we would not respond because to do so would, perhaps, initiate total nuclear war with many millions of civilians being killed. So it is a limited strike, and some advantage might accrue to it.

Then the answer has been what to do about it, and, of course, the answer is we develop a counter-force strategy, and that all fits into the on-going rationale of both sides towards the first strike capability, creating great instabilities, generally raising the likelihood, the real likelihood of nuclear war.

So let's just come back to the simplest scenario and ask what is the simplest thing that the Soviets might do if they

seriously wanted to conduct this limited counter-force strike? And I put it to you that if the logic is that they would like to make a minimum strike, directly killing the smallest number of people to lesses the chances of massive retaliation, but still to deliver a critical blow to U.S. nuclear superiority, I think the logic of that analysis would lead to the proposal to drop one nuclear bomb on the Livermore Laboratory; perhaps, a second one on the Los Alamos Laboratory. If that logic makes sense, that would be the best strategy, better than trying to take out five or ten minute men missiles, because there are thousands, hundreds, 900 -- what is it -- a total 1,054 land base missiles, so what is knocking out five or ten of those in a limited strike. But there are only two laboratories that are in the lead in the development of new nuclear weapons technology. So if there was logic to this, which is the country's first announced policy of what we are concerned about, the counter-force logic from the Soviet Union's point of view would be to drop a bomb on Livermore. And I suppose drop it at night when most people aren't there, so you haven't injured too many people and it is just one of these direct military related things.

So I would like to have it very carefully calculated and announced by the experts at Livermore what, in fact, would happen to that plutonium if, in fact, it would be vaporized and dispersed and cause millions of lung cancers? I think it might

be wise if our government announced that so the Soviets knew it, so that they would not do such a thing, because then that would be most provocative rather than a safe counter-force move. That is a kind of scenario that I have never heard discussed before, and I think to most people it sounds far-fetched. I encourage you to think about it, and see that in the light of the established logic of nuclear theorizing, it is not at all far-fetched. It seems to belong at the top of the page.

Let me now turn to the other end of the spectrum; that would be the minimum. I'm trying to estimate kinds of nuclear war risks that can occur. This was the minimum counter-force strike. Let me go to the opposite end, the maximum counter-force strike, a major nuclear war between the U.S. and the Soviet Union, and ask what would be the result.

And for this there is, in fact, a document I have here available, also prepared by the Department of Defense, and this was sent to me in response to an inquiry. It was over a year ago when I wrote to the Pentagon asking for a document on the subject of most likely targets in the United States in the event of nuclear war. This document is published by the Defense Civil Preparedness Administration. It is called TR-82, dated April 1975. The title is "High Risk Areas." It is a compendium of maps showing each state of the Union, with colored areas indicated on it. The colors are -- the red is the direct nuclear blast effects from a presumed and calculated Soviet

strike against the United States according to criteria which our Pentagon presumably uses in its own planning. The green areas are the fall-out areas. I think they say, high fall-out areas, and the brown is the overlap of the two. And this goes by state to state, indicating the targets that are presumed at risk in a major nuclear war. And, in fact, they list on the opposite page population at risk. They call them populations at risk within counties, total populations at risk for each state as a result of their maximum nuclear war scenario. And I am looking for California. Here it is.

One can see a lot of light area on the map. That is not included here. It may get low level fall out. But there are lots of red spots all over, and there are lots of green area. In particular, the entire San Francisco Bay Area is totally at risk. And on the right hand side are all the counties; the San Francisco-Oakland Area, 298 -- I'm sorry -- 2,987,850 at risk, and so on.

Now what I've done is late one night I just took my pocket calculator and added up all the numbers and got the total population of the United States at risk in this Defense Department analyzed scenario. The total number is 139,826,023. I think that was 1970 census figures I calculated according to.

Now I would like to take some of these numbers and incorporate them in a couple of graphs that I put on the wall over there in order to make an attempt to approach a comparison

of risks in two different areas. One area is the area that has been sovery thoroughly discussed in many arenas and the public is well aware of: the risks of nuclear power plants. And I'll put some numbers in there that are, I think, the government's own best numbers and compare to that the nuclear weapon risk from the nuclear war scenario that I just indicated here.

MR. FARMAKIDES: Dr. Schwartz, how do you relate the first chart to the draft environmental impact statement?

DR. SCHWARTZ: I think it is important to bring in the first chart about nuclear power plants, because in assessing risks it is always difficult to compare one kind of risk to another. If one is talking about dollars versus lives, as in many areas, those are incomparable things. It is very difficult. But I thought the use of them would be to compare the same type of calculus for one familiar area where a public awareness and sense of concern and desire for knowledge is well acknowledged with a relative risk in the other area, the nuclear war area, to show the relative import of these two. This, to me, then puts it on a rational basis rather than just purely abstract mathematics.

MR. FARMAKIDES: Let me understand that again. We are talking about a laboratory. We are talking about a laboratory that may have -- you quoted a certain amount of plutonium. Which of the two do you think is closely -- which of

those two scenarios that you are suggesting there are the ones that you are referring to with respect to that laboratory?

DR. SCHWARTZ: The one that I am going to put up here is the major nuclear war scenario which I just described. That is very analagous to what I am going to put in here. I want to draw scenarios that are analagous in two domains, so that people will have a sense of what they weigh, because numbers by themselves -- usually, it is just a number. People don't know how to relate it. I'm trying to draw parallels so that people can make -- ordinary people can make a meaningful comparison between a type of risk that has been familiarly discussed, and people have a sense on how they relate to it, compared to risks that have been so little discussed, and I want to put that in a context which then makes it, I think, useful.

I've taken a major nuclear plant accident on this side and a major nuclear war on the other side, and the figures, if I may proceed.

MR. FARMAKIDES: Here is my problem. If you are going to draw an H-bomb, I'm really way beyond my sphere of any knowledge at all. But if you are going to draw up an H-bomb on Livermore, it would seem to me that you'd have tremendous amounts of radioactivity from that weapon itself. Are you saying that the material, the radioactive material in the laboratory is going to augment significantly the radioactive of that weapon?

DR. SCHWARTZ: I would like the experts at Livermore

to do that calculation. And I think the answer might very well be strongly yes, particularly because a direction of nuclear weapons development that this country has been pioneering has been relatively clean weapons. You have heard that word many times. A weapon that will just do its job and not cause a lot of extra fall out. And the rationale for that is that such a weapon can be used in surgical type operations and be used in limited ways to achieve specific military objectives without raising the larger likelihood of greater nuclear -- well, that is exactly the logic being pursued.

MR. FARMAKIDES: Well, okay, sir. I'll wait to hear how you tie it in.

DR. SCHWARTZ: Okay. Let me give you then the famous numbers from the Rasmussen Report; the likelihood of a major nuclear plant accident is estimated at once in a million years. Now, of course, that number has been debated, but that is the standard number. The population at risk in the Rasmussen Report, deaths and injuries, the number, I believe, is 48,000 front, and 293, 000 delayed; deaths and injuries a total of 341,000.. And if we then compare those two numbers to write a nuclear power plant risk casualties per year, the total number over how many years the expectation is for it to happen -- this number divided by a million -- the answer is 0.3. Now I presume that kind of a number has been the basis for the statement nuclear power plants are safe. One can argue with the numbers,

but this is the standard reference for the assessment of the total population risk for the major accidents of nuclear power plants. That is just a reference for numbers.

Now let me talk about the analagous thing, the major nuclear war scenario. The number I gave you there, again, from the Department of Defense, population at risk, 139,826,023. Some science people might say why don't you round that off? I have a little theological feeling; the last digit here is a whole person, and I don't want to rub him off.

Now this is the hardest number here. What is the estimated likelihood of a major nuclear war? Once in how many years? Now that is the question that basically I would like to put to government experts, and I have asked for various experts, from the laboratories, from the Department of Energy and the Department of Defense to be here to answer that question. What is your best estimate of the likelihood of a nuclear war, of a major nuclear war, of smaller type ones? I'm sure this has been thought about, analyzed, and I would like to have authoritative numbers coming from the government. I have been seeing none. I will propose a number that I think I can defend on quite rational and logical grounds. I would say a few decades.

The argument for that is look at the trend in weapon redevelopment, the shift from counter-value, second strike capabilities to counter-force first strike capabilities. That

is a progression that has been occurring sharply over the last decade, gives every indication of continuing. It is very much increasing the likelihood of nuclear war. In fact, there is very strong reason to interpret that the U.S. policy in effect, though not announced, is to prepare our weaponry, not to avoid nuclear war but to be prepared to fight and win nuclear war. And the developments of the Soviet Union nearer those. And the result of this qualitative arms race is to make nuclear war more and more likely.

The time scale for the development of major new strategic weapons systems is five to ten years. So give me 10 years for the development of the super accuracy missiles; the multiple warheads we've already got. Give me another 10, if you want, 20 years for the further advancement of anti-submarine warfare techniques which seem to be making good progress already to erode the stability of the sea-base deterrent, making each side feel very jittery about a first strike capability. Give me another decade, a decade and a half for the increased improvement in the reliability of the missile system, the application of the shiva-nova laser fusion system to missile effects, the weapons effects studies, to improve the surviveability so that the scenario for effectively fighting a first strike warfare gets improved from 90 to 95 to 98 to 99.9 percent. And as the Soviets respond over these time periods of one or a couple of decades, the likelihood that the two sides are going to find

themselves trapped in a situation where nuclear war, striking first seems an awful but still the only way out, is then the likelihood of the war I'm talking about. So I propose a few decades, as a statistical likelihood on the basis of observing the present development of nuclear weapons, and what we are probably going to get is some fair probability of major nuclear war. And unless some other person here, expert or otherwise, would like to propose another, I'm going to write something like 30 years in here.

Does anyone want to propose a different number?

MR. FARMAKIDES: You can proceed, sir. These are your comments.

DR. SCHWARTZ: I know, but I think there are other opinions and I would like to see them. I'm going to write 30 years in there. I think it is a rational estimate.

The quotient then for total nuclear weapons risked, the casualties per year, this total number divided by that; that is somewhere like 4 million. I had 4,600,000, now rounding it off.

Now the comparison to be made is between the last figure. The total nuclear power risk, the total nuclear weapons here. The nuclear weapons risk is something like 10 million times greater. To me, that means there ought to be 10 million times more concern, energy, worry and protest about this development as compared to this development.

Do you want me to submit for the record those numbers?

MR. FARMAKIDES: Yes, I'll take them.

DR. SCHWARTZ: Now I should turn on my other part -- how am I doing on my time, sir?

MR. FAMAKIDES: You have, I would say, 15 minutes. You started at 1:40 p.m.

DR. SCHWARTZ: I think I am making good headway. I might even finish before that full time.

I would now like to make the linkage between this enormous risk, the threat of nuclear war that I spoke about and the actual activities of the Laboratory, because the posture taken in the staff reply was that this is national policy; it is determined somewhere else, and the Laboratory just carries it out. I would like to say that that is a very incorrect description of the process.

The Laboratories, and here I'm talking about in a very important degree their upper echelon of officials, weapons scientists into the management levels are not just people who follow orders. They are very inventive, creative people, who are actively involved in thinking up new weapons systems, in proposing those assessments to the government and, often, acting in a very active political way to convince parts of the government to fund, proceed, develop and eventually deplore those weapons systems. I would like to give some concrete historical examples of that behavior.

Perhaps, the sharpest one concerns the neutron bomb, an innovation in weaponry which from one point of view is going to make our security in western Europe safer. From another point of view, it has very much lowered the threshhold for nuclear war and endangered all of us.

A piece of testimony that I found, it was in the Congressional hearings in 1973 of the joint committee on atomic energy, entitled Military Applications of Nuclear Technology, of these hearings, April 16, 1973. The witness, Dr. Harold Agnew, Director of the Los Alamos Livermore Laboratory. This was partly censored testimony. But after what we now know about the neutron bomb, you can fill in the blanks. I will just read these two paragraphs.

I really don't know why people have not thought more on the use of these deleted weapons. It may be that people like to see tanks rolled over rather than just killing the occupants. I know we at Los Alamos have a small but very elite group that meets with outside people in the defense community and in the various think tanks. They are working very agressively trying to influence the DOD to consider using these deleted weapons which could be very decisive on a battle field which would limit collateral damage that is usually associated with nuclear weapons. This was then the active weapons promotion role.

We have, of course, heard a number of other occasions with no great surprise, the director and other officials of the

Livermore Laboratory going to Washington to pump for their budgets. It is a most natural thing for a bureaucrat. And bigger budgets, of course, come from new weapons programs, and they have been constantly seeking to enlarge the scope of their weapons work. There is a strange relation called competition between the Livermore and Los Alamos Laboratories that is supposed to increase the quality of the work. I think its main result is to increase the quantity of the work, wherein good old free enterprise the spirit, the scientists at each lab are, in fact, encouraged to think of a weapon, any weapon just to increase them in the race over the other laboratories. But, of course, the consequences fall upon all of us.

Not only in promoting weaponry but in working against attempts to reduce international tensions, to reach international agreements through treaties to limit armaments, the officials at the Livermore Laboratory have a long history of very strenuously lobbying against -- in the 1958 -1962 period -- against the atmospheric test ban treaty. And in the current years, again, both the Livermore and Los Alamos directors have been very heavily at work in Washington, lobbying mostly behind closed doors to sabotage the comprehensive test ban treaty which appears the U.S. and Soviet governments were finally getting close on, but then inventing, among other things, spurious technical arguments. But, obviously, out to protect their own turf and budgets, these officials have been quite effectively reducing,

sabotaging, at least slowing down the comprehensive test ban treating, thus, encouraging the continuation of the weapons development.

Also, in theoretical ways the officials and numbers, certain select numbers of scientists at the Laboratories are involved not just in technical studies but in military and geo-political studies to push the development of weapons, therefore push national policy in certain directions.

I have noted before, in particular, a paper by Dr. Michael May, the former director of the Livermore Lab, published in 1970 in which he gave an analysis of the inadequacy of the deterrent theory and advocate the move to counter-force strategy; that which we have, in fact, seen come about in recent years, the developments which I see very clearly are an acknowledgement that we are preparing in the country and the Soviets also forced to prepare to fight a nuclear war. God help all of us if that is forced upon us.

Now another implication of the staff report is --

MR. FARMAKIDES: Now before you go on. Let me understand that last point. That last point, your bottom line, as I understand you, is that the Livermore staff, the reason that they are promoting their -- in your words -- their budget and their turf, that that is a cost which has not been evaluated? Is that what you are saying?

DR. SCHWARTZ: It is the modus operandi of the laboratory in relation to the formation of "national policy." That

is part of a process that goes on in the laboratories and it is ingrained in their institutional arrangements, not in the machinery in the buildings but in their institutional arrangements. Its consequences are what we need to worry about. I think the consequences are what I've been pointing to at the blackboard.

MR. FARMAKIDES: Getting back to the draft environmental impact statement, your point, though, however, as you said earlier, I thought you said, is that that is a cost.

DR. SCHWARTZ: Yes, that activity creates a risk for the rest of us. Particularly so when we understand that that activity takes place mostly in secret under the rubric of national security; that it takes place largely amongst a group of people who form a rather isolated clique. Now, of course, the weapons people at the laboratory have to talk to officials in the Department of Energy and the Department of Defense to get things approved. But I think I have been able to show in some of my little biographical researches that it is the same set of people, a familiar pattern; weapons lab officials get promoted to positions in the DOE where they supervise their former colleagues, or sometimes in the Department of Defense. And you can see the officials from the Department of Defense in nuclear matters come then to the laboratories. The familiar military and industrial complex arrangement is very much at work here, except I think it is a much smaller, much closer

knit set of people who have been essentially running the nuclear weapons policy of this country with very little regard for the rest of the world. And from their point of view, I'm sure they feel that they are right and they are saving us all. But, if nothing else, I think my sense of democracy is offended by that arrangement.

As an example of this, I have a quotation from a very distinguished member of the Congress to indicated that, in general, these matters are not at all effectively reviewed by the Congress. One can understand, of course, when it is a highly technical matter, Congressmen will generally defer to experts. And the experts come from the two weapons laboratories, and they have the aura of the University of California name behind them, so they have this great appearance of independence, of legitimacy, of trustworthiness. But, in fact, they are people who have been professionally involved in weapons promotion and that is a highly selective, highly partisan point of view.

This quotation is by Sen. Stuart Symington from congressional hearings in 1973. And Senatory Symington, by the way, was in the Congress for a long time. He was Secretary of the Air Force under Truman. At least, in his earlier years a full fledged hawk, but, later on, he had some questions. This quotation -- he is opening some hearings. He says that he hopes these will be informative and constructive series of

hearings on the military applications of nuclear technology. And he said some have heard me say previously that not until I became a member of the Joint Committee on Atomic Energy and travelled to Europe with Senator Pastore in the spring of 1971 did I realize the true military strength of the United States and became acquainted with the vast lethal power of our nuclear arsenals. I actually learned more about the true strength of the U.S. forces in Europe in those six days than I had in some 18 years on the Armed Services Committee. One cannot help but consider the implications incident to our defense and foreign policies if the facts were known by the appropriate committees of the Congress as well as, in more general fashion, by the American people. And I hope those last words can be taken as an inspiration to you members up there to understand how important it is that these highly sensitive but very portentious issues should be made as much as possible subjects on the continuing public agenda.

I have -- and this may even be my next to last quotation. I would like to refer to an article in the July 1978 issue of Physics Today magazine, written by Victor Weisskopf. Professor Weisskopf is a physics professor at MIT. I had the honor to serve under him when I was doing my thesis. He was involved in the Manhattan Project, a very distinguished scientist. He was for some years the Director General of the European Center for Nuclear Research. I believe he also has

served in DOE. I think he was chairman of the high energy physics advisory panel for some years. A man with enormous credentials.

In this article he talks about the dangers of the nuclear arms race, and he makes some comparisons; again, not in a numerical way as I did but I think in a very striking way between the dangers and the concern that people ought to have over the dangers of the nuclear arms race as compared to the nuclear power. Let me read a couple of paragraphs, if I may.

The dangers and the promises of nuclear power generators are today in the center of discussion. Many studies have been undertaken and more are underway. Emotion invested interest unfortunately have lead to a sharp division of opinions, and the arguments used on both sides are too often beyond the limit of dignified scientific discourse. At the same time, the nuclear arms race between the super powers continues in an almost uncontrolled way. The Soviet Union and the United States assemble increasing numbers of bombs and perfect their efficiency and their mode of delivery. More than 50,000 nuclear bombs are deployed and ready to use. Each country now has the capability of destroying the other many times over. Current science is totally unprepared to discuss intelligently let alone to predict the totality of horrors that would result from an all out nuclear war.

He then tries to assess some of those awful consequences.

He says in comparison to this overwhelming threat, the nuclear power controversy dwindles to picayune dimensions. What are the dangers of nuclear power stations compared to the dangers of tens of thousands of bombs that can be released in seconds by a small group of human beings? What is the so-called worse reactor accident compared to nuclear war. The damage is done by the former which would come mainly from effective radiation, serious as they are, are far less than the effects of a single bomb. Now think of the number of victims of a nuclear war and its irreparable effect on our environment, on our souls, alive or dead, and on our whole planet.

And the probabilities? Nobody can really estimate the probability of an all-out nuclear war. But one fact is clear. With all those bombs around, it is not zero. Nuclear power may be too risky, or it may not. I do not pretend to know the answer. But I know that tens of thousands of stored bombs are too risky.

I had also in the packet I gave you -- and I'll just refer to them in passing -- two other articles by people of international repute, who are pointing to the growing danger of nuclear war due to the move toward counter-force capability, the particular development of the new technologies of weaponry, in which development the Livermore Laboratory is in the forefront.

One is an article by William Epstein, which was in

the June 1977 Bulletin of Atomic Scientists. Mr. Epstein was for many years the Director of the Disarmament Division of the United Nations.

The second one, also in the June 1977 Bulletin of the Atomic Scientist, is entitled The Mounting Prospects of Nuclear War by Frank Barnaby, Director of the Stockholm International Peace Research Institute.

I guess the third document I gave you was an expression by a large number of American scientists and engineers, a declaration circulated by the Union of Concerned Scientists in September 1977, pointing out the growing dangers of the counter-force race now under way. And I understand that has been signed by many thousands of scientists and professionals in this country.

I guess I will just close by repeating that I've asked a number of questions that I would like the government to respond to. I think the sharpest one is the one in the upper right hand quarter of my chart there. What is the risk of nuclear war? What is the risk of the varieties of nuclear wars that you can imagine? An all out strategic war? A limited counter-force strategic war, a tactical nuclear war in Europe or somewhere else? Or the occasional use of a war of a nuclear bomb on a third country situation, or the use of nuclear weapons by some other terrorist group or other small nation, and what are the couplings between these two? What is the expectation that a small nuclear usage escalates to the

large one? What are the details of the single integrated operations plan by which the Pentagon has all these questions for answer on a computer tape that will move us from Step A to Step B to Step C in a nuclear war scenario? I would like those questions answered in an authoratative and a very public way by the government.

And I guess the final question is that those of us who criticize the environmental impact statement are supposed to answer, what alternatives can we propose? And I hope you will find it no great surprise if I think the proper alternative is to stop the weapons research at the Livermore Laboratory and its companion one in Los Alamos. We have many better uses for the scientific talent, for the money and the equipment that are now deployed in a direction that I think ultimately and before too many years will destroy us all.

I thank the Board very much for your patience.

MR. FARMAKIDES: Thank you, Dr. Schwartz.

I think that with respect to your question, I'm just not sure that it is something that I can answer, or this Board can answer. I'm just not sure how that would be brought up into a public debates where there can be an answer. I terms of what this Board will do -- we are going to, as I've said before, we are going to discharge our responsibility which is to, in fact, evaluate the draft environmental statement, and view what you have said, and view what everyone else has said here

and come up with identification of those issues that we think are important.

DR. SCHWARTZ: I'm very pleased that you transmit our concern.

MR. FARMAKIDES: Thank you very much for your time.

I think there was one other person that said they would like to speak soon, after lunch, and that was Mr. Reynolds. Mr. William Reynolds.

MR. FARMAKIDES: Mr. Reynolds, could you kindly identify yourself for the record and proceed.

STATEMENT OF WILLIAM REYNOLDS

MR. REYNOLDS: Yes, I am William Reynolds. My address is P.O. Box 2234, I Point, North Carolina 27261. I am coordinator of the Nuclear Transportation Project of the American Friends Service Committee. American Friends Service Committee is a Quaker service organization which has been active in peace and social justice issues since 1917. The Service Committee has received international recognition for its work in peace and social justice issues, including the Nobel Prize for Peace. Our office has been involved in investigations of the transportation of radioactive material since the Fall of 1977. In my position as coordinator of the project I have been invited to appear before numerous city, county and state governments to discuss and brief government officials on the transportation of radioactive materials. In addition, I have appeared before the Department of Energy as well as the Department of Transportation to present testimony on future energy transportation needs and changes needed in the regulations of the Department of Transportation concerning the transportation of nuclear materials.

MR. FARMAKIDES: Mr. Reynolds, could we interrupt just one minute. I realize I've forgotten to announce those people that will follow you. Can I just do so for just a

moment. It's after yourself - Mr. Rothblatt, Mr. Soltzman, Mr. Andrew Baldwin, Mr. Rutherford, Mr. Goffman, Mr. Chaol. Go ahead, sir.

MR. REYNOLDS: I have reivewed the draft Environmental Impact Statement for the Livermore site and found it to be totally lacking in adequate assessment of the impacts, the environmental impacts of the transportation of radioactive materials. Only two sections in this statement even discuss transportation; Section 2.3.1 identifies the major highways, the two train routes and the one airport located in the vicinity of the Labs, yet gives no indication of how these routes are utilized by either Lawrence Livermore or Sandia Laboratories. Section 3.9.2 entitled "Transportation Accidents Involving Radioactivity" - unfortunately there is no discussion in this section of transportation accidents. This section summarily sets aside any discussion of transportation accidents by claiming, "The containers (those used to ship radioactive materials) are capable of withstanding the maximum credible transportation accident." This is on page 3-63. Now this is not only an inadequate discussion of transportation accidents but is blatantly false. In fact, Type A packages as defined by 49 Code of Fed. Regulations, Part 173, are only required to maintain their radiation shielding under normal transportation conditions. These packages are not required to survive accident conditions. This fact has been well demonstrated by numerous

transportation accidents which have resulted in the dispersal of radioactive of radioactive materials into the environment.

Type B packaging, as defined by 10 C.F.R., Part 71, Appendices A and B also are not required to withstand a maximum credible transportation accident. Type B packages are required to meet certain design criteria which model accident conditions. However, these are not maximum credible transportation accidents. In fact, the final Environmental Impact Statement on the transportation of radioactive material by air and other modes identifies a lack of test data on the safety margins applicable to Type B packages and also identifies six to ten percent loss of contents from Type B packages under certain transportation accident conditions. Now although the Livermore Environmental Impact Statement details the packaging standards, regulations and policies to be used for the transportation of plutonium by air, it totally ignores the transportation of all other types of radioactive materials, and I would like to say that the air transportation of plutonium materials makes up only a small percentage of the transportation of materials in and out of the labs.

There is insufficient information presented on exactly what radioactive materials are being transported, the quantities of materials carried per shipment, the destination or origins of shipments to and from the laboratories, the routes which are utilized, the mode of transportation utilized or the

number of shipments made.

On page 3-68 the statement says that most of the shipments made are by common carrier, yet this gives us no definition of what most is. In addition, it also does not identify how shipments which are not handled by common carriers are actually made. Not only is the information which is necessary for an adequate assessment of transportation risks unavailable in the Livermore statement, it is also unavailable in the public doman. The Federal regulatory scheme governing the transportation of radioactive materials allows for no public accounting of the movement of these particularly hazardous materials. Without more detailed information, it is difficult to judge the actual impacts of these shipments.

Now this fact is recognized not only by concerned citizens and environmental groups, but is also recognized by the over 60 city, county and state governments which have enacted regulations governing or restricting the transportation of radioactive materials in different locations across the country. I would specifically like to refer the Board to recommendations which were made by the state task force of the Resources Agency of the State of California concerning the transportation of radioactive materials. Recommendation 6-26 states that the Department of Defense and the Department of Energy should provide a specific accounting of their most hazardous radioactive material shipments to the State of

California. Recommendation 6-26 calls for an agreement to be executed with Department of Energy facilities for advance notification of radioactive material shipments which are exceeding a certain hazard threshold. Information which would be provided by a specific accounting of all radioactive material shipments and advance notification of these impending shipments is needed to fully and publicly assess the impacts of these hazards.

Now I will attempt to outline some of the major types of shipments that are made into and from the Livermore Labs. In Section 3.5.1 the statement identifies five major producers of radioactive wastes at Lawrence Livermore Labs. These are the pool-type reactor in building 281, the linear accelerator in building 194, the heavy elements chemistry building 251, metallurgy chemistry building 332 and the light isotope chemistry building 331. The statement further identifies three types of radioactive wastes produced by these facilities. One would be low-level wastes consisting of contaminated rags, gloves, other articles of clothing and filters, as well as activated metal components coming from within the reactors or the accelerators. Another category would be transuranic wastes. These consists of wastes containing plutonium and other elements heavier than uranium which are typically long-lived and very toxic. Then there are finally liquid wastes which are usually treated to produce either low-level wastes or transuranic wastes for handling on site.

Now all of these wastes are stored on site temporarily and then when sufficient quantities are gathered together transported to other sites for final storage.

According to the statement, low-level wastes are transported to Beatty, Nevada for burial and the transuranic wastes are taken to "Department of Energy site for long-term retrievable storage." Now presumably this would be Hanford, Washington, although it could possibly be another facility as well.

Now additionally produced, but not even discussed in the Environmental Impact Statement are high-level wastes in the form of spent fuel rods which are produced by the pool-type reactor in building 281. Spent fuel requires special handling and storage procedures due to the high levels of radiation which are emitted from these materials. There is no mention in the Environmental Impact Statement of the quantities of spent fuel produced and methods used to handle and temporarily store the spent fuel, the final disposition of the spent fuel, nor even the fact that they are actually produced.

Now also produced but not discussed, actually also made to the site but not discussed are shipments of strategic quantities of special nuclear materials. And this would be Plutonium 239 and Uranium 235 and Uranium 233. Now these shipments are of a classified nature and are made by special courier teams of the Department of Energy.

No analysis is presented of the accident probabilities

of these shipments, nor estimated impacts from a postulated maximum credible accident. In addition to possible accident scenarios, an analysis of possible terrorist activity on these shipments is also needed.

I mention this specifically because although an Environmental Impact Statement has been prepared on the transportation of commerical radioactive materials, there has been no Environmental Impact Statement on the transportation of radioactive materials utilized for defense purposes, either by the Department of Defense or by the Department of Energy.

Now there are also shipments of numerous radioactive isotopes from other Department of Energy Labs which are made into the Livermore facility. The majority of these originate from Oak Ridge operations and have included such isotopes as Curium 252 and 245, Neptunium 237 and 239, Protactinium 231, Einsteinium 253, Crypton 85, Tritium, Thorium 236 and 232 and various isotopes of plutonium and uranium.

Other shipments have also originated from the Hanford Reservation in Washington State, the Mound facility in Ohio and Batell Pacific Northwest Laboratories, also in Washington State. And these have included such isotopes as Amarisium 241, various isotopes of plutonium, neptunium and also depleted uranium.

Now in summary, I would like to say that the section of the draft Environmental Impact Statement which discusses the

transportation of radioactive materials needs to be completely rewritten in order to present a thorough and complete assessment of the potential and existing transportation impacts of radioactive materials. These should include a full accounting of the radioactive shipments to and from the laboratories, including the number of shipments made, the quantities which are included in individual shipments, the origin and destination of shipments and the route that the shipments are following.

There should also be a full assessment of the probabilities of accidents, including involving radioactive materials. As well, there should be estimates of the impacts of a postulated maximum credible accident of materials being transported into or from Lawrence Livermore Labs. Now this can be done as well by reference to existing reports, as well as by independent calculations specific to this particular site.

There should also be a discussion of possible procedural changes which could be made at the site, including notification of impending shipments to local authorities, particularly government authorities as well as emergency response personnel; that is, response personnel in local communities who would have to respond immediately to an accident involving these.

There should be discussion of involvement of local agencies in planning for which routes should be followed to ship these radioactive materials so as to minimize the potential

impacts, and there should be some consideration on on-site storage of the spent fuel as well as other particularly hazardous radioactive materials until such time as a permanent disposal site has been demonstrated so as to minimize the impact of transporting these materials to other facilities which may simply be temporary facilities which would require that they be transported again in the future.

Now outside of the realm of transportation, I also had some particular concerns about a couple of other sections in the statement. There are two references under the benefits of the operation of the Livermore Labs. In particular, one identifies the benefit of national security from the operation of Livermore Labs in the production of new types of nuclear weapons. This is identified in Section 2.2.1.

Here I would question whether or not there is a true benefit to the United States from the production of new types of nuclear warheads, particularly in light of the fact that even with the reduction in our present nuclear stockpile, we could still maintain a credible nuclear deterrent in the event of an attack by a foreign country.

Secondly, it identifies economic benefits which are made to the Livermore community by the fact that there are 5,000 or more employees at the Livermore site, 50 percent of whom live in the Livermore area.

Now I question whether or not this benefit actually

exists solely as a result of the operation of the facility for nuclear weapons production. And this leads me into my final concern which is that there was inadequate discussion of the alternatives to the operation of the site.

And here I would like to point out that particularly lacking is a discussion of the conversion of the facility to applications other than nuclear weapons design, which would maintain the employees at the site and maintaining the economic benefits to the local community.

In fact, the NEPA Act, in its discussion of how programs should approach looking into alternatives of possible projects, defines that there should be a rigorous investigation of possible alternatives to the implementation of any given project. And here, I would submit that the alternatives which are discussed in this particular impact statement clearly are not a rigorous investigation of other possible alternatives to the site. I think that it would be very productive to have a very rigorous examination of other uses of this facility, particularly in regards to the conversion of the facility to non-nuclear applications.

Thank you.

MR. FARMAKIDES: Thank you, Mr. Reynolds.

MR. BEARD: I have one question related to the main part of your argument. It is only for my comprehension. Except for the storage of the reactor fuel oil elements, your

concern is primarily off-site transportation? That is what you addressed yourself to?

MR. REYNOLDS: That is correct. Primarily, the off-site transportation of radioactive materials.

MR. BEARD: Thank you, sir, very much.

MR. FARMAKIDES: Next is Mr. Harlan Rothblatt, then, Mr. Stolzman, Mr. Baldwin, Mr. Rutherford, Dr. Gofman.

STATEMENT OF HARLAN ROTHBLATT
U.C. NUCLEAR WEAPONS LABORATORY CONVERSION PROJECT.
BERKELEY STUDENTS FOR PEACE

MR. ROTHBLATT: My name is Harlan Rothblatt, and I am with Berkeley Students for Peace and the U.C. Nuclear Weapons Labs Coversion Project.

I would like to adress a few different concerns, and all of them stem out of a concern that things have been left out of the Environmental Impact Statement which belong here.

First, before anything else, I would like to ask two specific questions about concerns which are not addressed in the EIS. First of all, there are no accidents listed in the draft Environmental Impact Statement, no accidents listed as having occurred before 1960. However, between 1960 and 1970, there were a total of 8 accidents involving nuclear materials and releases of radiation, which averages out to about one accident every 15 months. This is in the Appendix 3 C.

It is statistically improbable that in a ten year period there would be eight accidents, or one accident every 15

months, but that in the entire existence of the laboratory would be prior to this period that there would be no accidents involving nuclear or non-nuclear materials.

Therefore, I put the question to the laboratory. Were there any accidents prior to 1960 at the Lab, and, if so, where were they, when were they, what were they, and what did they involve, and how much radioactive material did they involve?

Second of all, there is only one land site for low level radioactive waste mentioned in EIS, and that is Site 300. I would like to know if there are any other sites which, at any other time, radioactive substances, low level wastes or materials contaminated by radioactive substances have been buried or stored within 100 miles of the Laboratory. If so, where, when, how much material was stored there or buried there, and is the site still in use?

There are at least two very major areas of concern involving radiation dangers, health hazards, directly coming from the Laboratory which are not covered in the Environmental Impact Statement, and these are weapons testing and leakage of radioactive materials or radionuclides from dump sites near Farallon Islands, about 30 miles off-shore from San Francisco.

Other people have addressed and will address the question of weapons testing in Nevada, so I will address my remarks to the release of -- their continuing release of radionuclides into the marine environment around the Farallon Islands.

Between 1946 and 1965 there were at least -- this is the Livermore. I would like to show you this document which is from the Laboratory about radionuclides being released off the Farallones. Their assessment of how many barrels is 47,500. I've heard higher estimates but --

MR. FARMAKIDES: Would you identify that document?

MR. ROTHBLATT: This is document UCRL 52381, dated January 6th, 1978, entitled "Radionuclides in the Marine Environment near the Farallon Islands," and it was a Lawrence Livermore Laboratory publication which was prepared for the DOE. It says, prepared for U.S Energy Research and Development Administration, ERDA.

Their estimate is 47,500-55 gallon drums of low level wastes were dumped in several sites off the Farallon Islands, and they estimate that there were 14,500 curies of thorium, uranium and mixed fission products, including plutonium involved in this waste. Approximately one third of these barrels are now leaking. This is not mentioned in the report, but they do mention that barrels have imploded. They quote other sources that are studying leakage.

They quote a study done by R.S. Dyer in 1975, who made an analysis of sediment sections from the sites, the dump sites, and found that the radioactive levels there were two to 25 times higher than background fall out levels. It was also found that the currents coming from these sites move north and

come up against the shore around Pt. Reyes and south of Bodega Bay.

In the National Environmental Policy Act, Section 1021.23, Pare E, I quote: EIS's covering a site under DOE jurisdiction shall assess the individual and cumulative environmental consequences of a number of continuing and/or proposed actions at the given site. It does say in this report that plutonium is being re-mobilized; this is continuing contamination. It is against the law, therefore, I submit to you, that the Farallon Island radionuclide contamination not be included in the Livermore EIS.

Finally, going along this line of things which have been omitted. This is not something which has actually been omitted, but there is a question about who is right with respect to what is in the EIS; what is an omission and what is not. Earlier, Mr. Barlow said that there are studies which have been done which show that the area around the Labs and around crucial buildings involving radioactive materials can undergo a 0.8 g ground movement. And there was some discussion with Mr. DuVal, and he maintained that they have looked at other studies which maintains 0.5 ground movement. The question becomes well, who is looking at which studies.

I think there is a severe issue here of credibility as mention by Mr. Riggin before me. And I would like to use, as an example of questions that I have about credibility, some

things which came up in this study about the Farallons, where they were they were mentioned, to show the kind of way in which the Laboratory goes about assessing what is environmental hazard, what is a health risk.

There are measurements of sesium 137 levels in rock fish and also in policate worms that they did in this study. They neglected to include radiation levels for aquatic plants, and they also neglected to mention giant sponges which I believe to be growing on these drums of waste and which have received a considerable amount of media attention; they simply omitted that mention of the sponges.

To show us that the sesium 137 levels in the rockfish were, I guess, acceptable or tolerable, they pulled out these rather obscure comparisons; one was to rockfish in Chicago which had slightly higher levels of sesium 137 in them, and one was to albacore who were three years old or older. Albacore migrate up the California coast. Which had a higher amount of sesium in them. As if this was supposed to mean that it was not really being a contamination of fish due to the Farallon site. I think that is a kind of fishy comparison to have had these. One wonders why they did not take the same kind of fish from a comparable area near the site and measure the contamination that way.

That is basically what I have to say except that I would like to reiterate that there is a severe credibility gap

here. For my mind, I asked those two questions at the beginning. I have serious doubts about how I can assess whether I have gotten an honest answer to those questions. I really don't know how to go about that.

It seems to me that the present way in which environmental risks and hazards are being assessed by the Lab is inadequate, and it seems to me that a certain amount of cover-up and self-protection is going on, as evidenced in this decision about which study to use concerning the g's for seismic activity.

Thank you very much.

MR. FARMAKIDES: Let me ask the staff those two questions. Your first one went to the number of accidents recorded before 1970, was it?

MR. ROTHBLATT: There are no accidents mentioned as having occurred before 1960. I want to know if there were any because it seems unlikely that there weren't if there were so many between --

MR. FARMAKIDES: The Appendix goes to 17 total, as I remember. I read this some time ago, but 17 --

MR. ROTHBLATT: I stopped at 1970. I was just considering the ten year period.

MR. FARMAKIDES: I see. You went up to 1970?

MR. ROTHBLATT: Well, it is covered -- the accidents start in 1960, and they go all the way up -- and I want to know

before, right.

MR. FARMAKIDES: Mr. DuVal, do you know, sir? Could you respond to that question?

MR. DU VAL: Mr. Chairman, the Appendix 3 C that has been referred to is a listing of accidents at Lawrence Livermore Lab which either had off-site impact or the potential for such impact. This does not purport to be a complete listing of all accidents on site, but those that have off-site impact. In that sense, it is complete with the exception of one accident that the media certainly brought to everyone's attention, that came into being as a result -- beyond the printing date of this report, and will be included in the final one, the amarisium in the --

MR. FARMAKIDES: That was the August 1978?

MR. DU VAL: Yes.

MR. FARMAKIDES: That's in the supplement?

MR. DU VAL: That will be picked up in the final report, of course. But, with that exception, this represents all that had the outside impact, and that is all this listing intends to be.

MR. ROTHBLATT: Can I ask Mr. DuVal a question?

MR. FARMAKIDES: Ask us. What is it?

MR. ROTHBLATT: Well, it is just a clarification of what he just said which is, therefore, I am to understand that even though there was an average of one accident involved with

off-site impact, or potential off-site impact every 15 months between 1960 and 1970. It got a little better after 1970. But one every 15 months,for a period of six or eight years before that there was zero such accidents. Is that what you are saying?

MR. DU VAL: You selected the 15-month period to determine the frequency of accidents which had either off-site or potential off-site impacts. For these accidents listed here, one would therefore divide by the interval from 1952 to the present.

MR. FARMAKIDES: In other words, the answer to your question, as I understand Mr. DuVal, is that that is correct; that there were none prior to 1960 that had an off-site impact.

Is that right, Mr. DuVal?

MR. DU VAL: Yes, sir.

MR. BEARD: I have one question. Your dump sites off San Francisco apparently originated -- the waste material in this Lab?

MR. ROTHBLATT: Much of it, and I have not been able to ascertain exactly how much of it came from Livermore, but I understand that a great deal of it did come from Livermore.

MR. BEARD: Let me follow that further. I didn't get your recommendation. You want it removed, or I thought I got your recommendation that you only wanted it included in the report. Can you clarify that? Do you want the material

removed out of there, or you just want the fact that it was there included in the report?

MR. ROTHBLATT: Well, I think that that is a good idea, that the material be removed. In that sense, you improve upon my suggestion. What I was saying is at the very least it seems to me, by law, by reading of the law, that because, according to Livermore's own data, and here there is leakage, and plutonium is re-mobilizing and is getting into the environment, and fish are being contaminated with sesium 137, and, therefore, it certainly should be talked about in the environmental impact statement because it is having a severe environmental impact on people who live in the Bay Area.

There was one newspaper case that I remember personally reading about, where a red snapper with an abnormal amount of sesium 137 turned up at a Berkeley fish market and was carted off by some government officials. I don't know how they originally found out that it had that content.

MR. BEARD: You want it in the report?

MR. ROTHBLATT: I certainly do.

MR. BEARD: All right. I'm trying to be clear.

MR. FARMAKIDES: Your second question, sir. Could you restate that?

MR. ROTHBLATT: The second question was -- Site 300 is the only site that was mentioned as a land site within 100 miles of the lab, and I would like to know if ever there

was another site and where it was.

MR. FARMAKIDES: Mr. DuVal, could you respond to that, sir?

MR. DU VAL: With respect to the Farallons, Mr. Chairman, I would like to make a comment.

My understanding is that the Farallon dump site was a licensed commercial respository or -- site, that operated up to the mid-sixties for commercial waste, low level radioactive waste for this region or this area. And, as such, while Livermore Lab was a contributor to the low level wastes there, so were the other operations at other laboratories and commercial generators of low level waste. That activity has not been in operation since the mid-sixties. I understand it has been surveyed by EPA now on several occasions. They indicated on their last survey that I am aware of that there was not a hazardous level of activity associated with it.

But the more particular comment, Mr. Chairman, is that it is not a part of the Livermore site operations. It was a commercial dump in its time.

With regards to sites other than Site 300, if I recall your second question, within 100 miles of the laboratory, where there is buried or stored, the only area that I'm acquainted with is the commercial site in Nevada. I can have the staff check and see whether there are others. But I am not aware of any other at this time.

MR. FARMAKIDES: That is the only one which the statement articulates.

MR. DU VAL: Beady, Nevada low level site, which is identified in the DEIS.

MR. FARMAKIDES: Do you have any information to the contrary, sir?

MR. ROTHBLATT: That there have been other sites? I don't have any hard facts to the contrary.

MR. FARMAKIDES: Would you check then, Mr. DuVal? Do you have any member of your staff here that can respond to that now?

MR. JACKSON: I'm Calvin Jackson, and I'm Director of Environmental Safety for the San Francisco Operations Office.

Low level waste as generated by DOE operations is routines sent to Beady, Nevada Nuclear Engineering Company, and except for depleted uranium, which is a by-product of one of our operations at Site 300, that is the case, that is in every case. We have no other situations where any material is deposited or stored on site in a long term operation.

MR. ROTHBLATT: My question, just to clarify it though, because I don't think you quite answered it, was not whether this is a practice which is engaged in now, but whether there ever was at any time, even if it is no longer in use, another site?

MR. FARMAKIDES: Do you know, Mr. Jackson? Do you

know if there ever was another site?

MR. JACKSON: Not to my knowledge.

MR. FARMAKIDES: Mr. Du Val says the same.

MR. DUVAL: With the exception of the commercial site at the Farallons that was discussed.

MR. FARMAKIDES: All right, sir. Do you have another question? Thank you, sir.

I have had presented to me now a document, the Cancer Hazard from Inhaled Plutonium, John W. Gofman, May 14, 1975, and, also, the curriculum vitae, March 1977, John W. Gofman, M.D., Ph.D., and comments on the environmental impact statement for the Lawrence Livermore Laboratory. We will accept these into the record as an exhibit submitted by Dr. Gofman.

I'm sorry. It has taken us a little longer than we initially thought, but that is something that we can't control.

STATEMENT OF ANDREW BALDWIN
LEGAL DIRECTOR, FRIENDS OF THE EARTH

MR. BALDWIN: My name is Andrew Baldwin, and I'm the Legal Director of Friends of the Earth. And the next five or six names that appear on the agenda are people that Friends of the Earth has requested to speak today.

MR. FARMAKIDES: Are you speaking in lieu of all of these people?

MR. BALDWIN: No, not at all. I've asked them all to come, and I will call them in order.

MR. FARMAKIDES: All right, sir.

MR. BALDWIN: As I've said, my name is Andrew Baldwin. I'm an attorney, and I'm legal director of Friends of the Earth. FOE has tried, or has been trying for some time, for a period of years, to get the plutonium operations removed from the Livermore site.

It is easy to understand the diligence with which the Lawrence employees build weapons to kill millions of Russians, but it is much harder to understand the indifference with which the Laboratory may cause the death of thousands or millions of Americans if there is ever an earthquake out here.

Now I have a document called the final safety analysis report for Building 332, the plutonium metallurgy building. And on page 35 this document says that typically in process Building 332 contains about 40 kilograms of plutonium. Much of this material is in metallic form and it will ignite spontaneously if it is ever comes into contact with air. And we heard today about an elaborate system of concentric negative pressure which always sucks leaks inward and banks and banks of redundant heaper filters to prevent the release of any of this material into the outside environment.

Well, all of this fancy engineering is not going to be worth anything if the earthquake comes. At least, we don't think that they can assure that it will be.

The design basis earthquake for Building 332, as far

as we can tell, is a .5 g with no off-set. In other words, they do not believe, or they did not design for the possibility that the ground underneath the building could slide. And, yet, there are a number of reports prepared for the laboratory which indicate that a .5 g with no off-set is not a conservative design basis earthquake.

The so-called Burner Report from 1972, design basis earthquakes for the LLL site, UCRL 51193, postulates a .8 g. The seismic appendix, surprisingly enough, of the draft environmental impact statement done by Mr. Larry White, postulates a .8 g.

At the Vallecitos site across the hill, the Nuclear Regulatory Commission facing a geologic situation remarkably similar to the one here postulated a 1.0 g, in other words, twice as high as the design basis earthquake for Building 332. And, finally, we have a letter today from Dr. James Brune, a seismologist from the Univerisity of California, San Diego. Dr. Brune says that you can get 2.0 g at Livermore, four times as high as the design basis earthquake.

Perhaps even more significant is that the possibility exists that there will be surface off-set at the site. The Lawrence Livermore Lab first said -- in other words, the first map that they published that we found shows two earthquake faults; one called the Corral Hollow Fault and one called the Tessler Fault crossing the site. This was in UCRL 51193 that I

cited before.

Well, structural engineers will tell you, and will probably tell you today that you cannot design a building for substantial surface off-set. You cannot assure that that will not damage the building, especially in the case where the building not only has to remain upright but has to remain airtight. So it is a little uncomfortable to propose that surface faulting may occur around here with all this plutonium, so they redrew the map.

And in the Draft Environmental Impact Statement -- this is my personal favorite of all the Livermore documents. There are four earthquake faults which come virtually up to the fence of the Lawrence Livermore Lab and stop. This is on page 2.a.17 of the green book. So they have eliminated surface faulting as a design basis from the design basis earthquake based on the analysis that the earthquake faults come up to the fence and stop.

At Vallecitos, we should note that, again, in a geologic structure remarkably similar to the Lawrence site, the Nuclear Regulatory Commission posited a possible off-set underneath the GE test reactor of 8 feet. What we have to assume, therefore, and the people who are going to follow me will, perhaps, reinforce that impression, that severe damage to Building 332 is an event of some probability in event of an earthquake. And we should take a brief look at what that means. Because if

a warehouse falls down, you have to rebuild it. And if a school building falls down, you lose some children. But if Building 332 falls down, that would be a holocaust.

There are 40 kilograms of plutonium in the building and much of it is in metallic. Metallic plutonium will often burst into flames spontaneously on contact with air, and this would produce a plume of plutonium oxide smoke.

Well, Dr. Gofman, who was here just a moment ago but had to leave, has done some calculations on the toxicity of plutonium. And plutonium is named for the devil and it is well named. It is capable of causing cancer well in the range of one microgram, one millionth of a gram, 7 billion cases per pound, if 40 kilograms represents 100 pounds. Now this is assuming perfect inhalation.

They gave some plutonium to some dogs one time, and these does were in the microgram range. At the lowest dose they were able to give the dogs, all the dogs got lung cancer. Considering the toxicity of this substance, that it is going to be here for 250,000 years, I think we should probably imagine that that plume of plutonium oxide smoke would be the breath of the devil.

Friends of the Earth has asked a number of experts to appear today. We have given them no compensation. We have no connection with any of them. And we believe they will demonstrate a substantial uncertainty as to the ability of

Building 332 to take it.

The toxicity of plutonium, the data that I gave you about, pretty well accepted data about how bad this stuff really is, gives us some indication of the magnitude of the disaster that a plutonium fire in a damaged Building 332 would mean. And, therefore, we again ask, as we have asked again and again over the years, to get that plutonium and the other actynides off this site. This is a seismically active area, and the Department of Energy is really messing around with the devil. And they are doing it, and by doing it, they are putting other people, the 5 million people that live around here at risk and their generations for thousands of years to come.

With that, I would like Mr. Stolzman to come up and make a few comments about the site geologies. Mr. Stolzman is from the Lawrence Berekely Laboratory, and he has made a preliminary review of the site.

STATEMENT OF ROBERT STOLZMAN

MR. STOLZMAN: I think one of the important things that you people should realize is the uncertainty in estimating these numbers. People think that scientists have the answers and you say a number 1 g, 2 g's, well, that is absolute. Well, it is not.

First of all, you should realize the techtonic environment we are dealing with. You have the whole North American plate riding over the Pacific plate with a spreading center

going underneath California. It is the most seismically active area in the U.S., possibly, the world. The scale of forces at work here are just continents. It makes the power of H-bombs look insignificant.

These faults in the Livermore Valley are most likely intimately connected to this activity. You have compressional forces coming in from the southwest and the northeast pushing.

Okay. Now you want to know whether these faults are active. There are many ways you can tell. For instance, historical seismicity, surface faulting and so on across the strain. But really not enough is known for successful determination of this activity. And, in most cases, the recent movement can't be dated, so you are guessing once again.

In looking at earthquakes, when you are locating the epicenter, usually the error in locating this is greater than the distance between faults. So you may think one fault is giving you an earthquake, whereas in actuality the fault next to it is giving it. So, once again, there you have more error.

Another thing, the geological history of this valley is an in-filling of sediments. So you are not really building on bedrock. Most of these faults are forces at work coming from bedrock. And you can get manifestations from different areas of this alluvial fill. You can shift.

For instance, the fault that Hurd found in Vallecitos, people think that, well, he is right, and the people who saw it

earlier were wrong. That may be the case. It may be the most recent trace has moved. There would have been more than one trace, more than one manifestation of this fault. So, once again, more uncertainty.

Also, according to geodetic evidence, some studies done in the late 60's, the depression and shearing in this valley is continuing today, and there is differential stress across the faults, so the area is certainly seismically active.

Another question to be raised is how long have we been looking at these faults, how long have we been monitoring them? As a fact, most of the seismic equipment has not been developed except since the early 30's, and most of the equipment for monitoring small, micro earthquakes, 4, three and a half on the Richter Scale or below, has not been around since the early 40's only.

Okay. You are looking on a geological time scale. The earth is four and a half billion years. You are talking about looking at 50,000 to 350,000 years to determine if earthquakes are, in fact, active.

Assuming people paying attention to earthquakes have been around, say, 200 years, this is no indication of what you can expect as far as a maximum earthquake. Getting down to putting numbers on this. Looking at past earthquakes, you can relate magnitude with rupture length and displacement. But as far as predicting earthquakes in the future, you don't know

what this displacement is. So the only way you can relate this is relating fault length. When a fault ruptures, you don't always get the full fault length rupturing. You may get anywhere from a small percentage to up to a third or more. Fine. What number do you use then? Also, are these faults connected?

You look how these faults trend toward the Calaveras Fault. You look on some of these maps. They have dash lines. People really don't know. On these maps -- they look so nice an clear. You go out in the field and they can be anywhere. It is very difficult to tell. So when you are using this parameter of fault length, you are using a guesstimate. You are saying I think this. Then you are plugging it into an equation and getting a hard number out which gives you an uncertainty.

Another uncertainty is when you look at the fractional stress drop which is the total amount of stress building up in a fault as compared to the amount of energy that is released when this fault has a quake. For below magnitude 7, this fractional stress drop is very small. It is fairly well below 20 percent. It is also very similar for earthquakes in the range of magnitue 4 to magnitude 6. The point I am trying to make is that with a certain amount of stress you can get a magnitude 4 earthquake, you can get a magnitude 6 earthquake. There is very little way of telling. There is a lot of micro-seismic activity in the area, but these quakes don't act as

safety valves because the stress drop is so small.

Looking at the maximum possible earthquake, if you look at the quake of July 4th, 1861, it had an intensity of 9, which is a measurement of how things were felt when the glasses rattled and so on. This intensity --

MR. BEARD: Richter or McCalley?

MR. STOLZMAN: McCalley intensity of 9 indicates that the materials in this valley can support a Richter intensity of 6 to 7. Six to seven is already a devastating quake, considering how close some of these faults are. It could be quite serious.

And, once again, just to say that this area -- if you look at some of the papers written on the area, there have been quite a few micro earthquakes. It shows that the valley is readjusting to techtonic stress and is certainly active.

The final point that I want to make of all that I am saying is it is very difficult to say that you will not not have a quake higher than 4 or higher than 5. You can say well, you will get this and so- on, but it is always possible to have more than you expect.

MR. FARMAKIDES: Thank you, sir. Dr. Grose has a couple of questions.

DR. GROSE: Mr. Stolzman, you indicated quite a few uncertainties here regarding earthquake prediction. And I wish you would indicate what, in your mind, the most important

view of these uncertainties are in the long list you gave us?

MR. STOLZMAN: When you are trying to put a number on a maximal, expectable earthquake, the only thing you are relating it to -- at least with numerical methods that they have today -- is fault length that you expect to rupture as related to magnitude. If you can imagine this on an x-y plot, you would have say fault length on the x axis, largest expected magnitude on the y axis, and you draw a line through these points -- there is a huge scatter. It is not like a nice, neat thin line. It is like it is all over the graph. And you say where can I put this line so it fits best. That is my main objection to putting a number on this.

Also, they say that there are certain maximum earthquakes that are possible. I say that based on this quake of July 4th, 1861, you can't support a 6 to 7 earthquake which would give larger than a .5 g acceleration. I think that's my main point I am saying.

DR. GROSE: Would you care to clarify what should be done about this situation?

MR. STOLZMAN: Certainly. Personally, I'm against weapons production, but I am not a politician so I don't want to make recommendations there. I think the least that should be done is that it should be moved away from this country. This is the most seismic area in this continent, and, at least, get it out of here, someplace less seismic.

DR. GROSE: There are reported to be studies underway and studies planned for the future to perfect our estimation for earthquakes here and elsewhere in the world. This may be an appropriate time for some of the officials of DOE to make some comments on this point. It has been brought up several times here today. This may be the time for a few words on what you are doing and what you are planning to do to improve what we know about earthquakes here in this part of the state and very close by.

MR. FARMAKIDES: Mr. Du Val, could you take that, or give it to whoever else in your group might know the answers?

MR. DU VAL: I'll start it, and we will try to cover that with the others that are here too.

I would like to point out to the panel the point about the conservative view of the degree of rupture length of the fault line as Mr. Stolzman reflected is, in fact, fairly well laid out in the staff study and the staff response, in terms of that being the difference in large degree between the .8 g versus the .5. The level of conservatism, the way in which -- the estimate of the total length of the fault that could rupture is precisely the same point in terms of the approach that was taken, that Mr. Stolzman talked about.

In terms of the additional work that we have identified, it is summarized in the staff study.

Let me ask Mr. Olsen to -- either himself, or if he

wants to call one of the seismic specialists here to give a little more detail than is contained on page 16 of the staff response, which does summarize the additional work that has been identified in the seismic area. Do you want to do it, or do you want to ask --

MR. OLSEN: We'll ask Jim Scheimer to present our program.

MR. SCHEIMER: We've already started. My name is Jim Scheimer. I'm a seismologist at Lawrence Livermore Laboratory. If you want credentials -- bachelors and masters degrees from Stanford University, Ph.D work at the Massachusetts Institute of Technology in theoretical seismology.

I'll just run through what it is that we are doing and planning to do. If you have further questions, address them to me.

We are proposing and have started a geological investigations which include but are not limited to -- let me make that clear -- a reevaluation of all available, pertinent data; that means searching out files in the various county files and state files, air photo analysis, trenching and drilling operations at the site, and surface mapping of geologic structures. In addition to this, we are proposing hydrologic analysis, including various well tests and geo-physical exploration. Geophysical exploration will include electrical, magnetic, seismic, geodetic and gravity studies. And, lastly, to lower

these error bars on what is active, where the activity is, we are installing a more dense seismic net in the Livermore valley to tie in with the United States Geological Survey, a regional California net so that we may locate activity much more closely than has been possible to do up until now.

MR. FARMAKIDES: Thank you, sir.

This is one of the issues that the Board itself has identified as being -- what we consider to be a critical issue, and you've articulated some information. And I hope that during the course of the rest of the day we'll get other pieces of information. It is definitely an issue, and it is a serious issue.

MR. BEARD: I guess I have kind of a silly question. Would you be satisfied with a 2 g design rather than total movement, or 5 g.

MR. STOLZ: It is my attitude that it is very difficult to put a maximum limit on this. It seems to me that no matter what it is not worth the risk. If you have other facalities where you can move this, where it is not seismically active, where you can just cut this risk to zero.

MR. BEARD: So there is no g you would be personally happy with?

MR. STOLZMAN: Exactly.

The whole theory of plate techtonics which has just revolutionized most thought in geology has only been around,

really accepted in the past 10, 12 years. Who knows what is coming along now? I don't think that we should be so arrogant as to assume that we know how the earth behaves.

MR. FARMAKIDES: I have a question. One of the debates appears to be whether or not there can be a surface rupture. What is your analysis of that possibility?

MR. STOLZMAN: Whether there can be?

MR. FARMAKIDES: At that point along the Tesla Fault.

MR. STOLZMAN: I wouldn't venture to guess. I suppose it is possible, but --

MR. FARMAKIDES: Mr. Baldwin earlier said that that is a distinct probability. I think Mr. Baldwin, and I am just wondering what you think.

MR. STOLZMAN: The danger of a surface rupture, even with a small quake, even if it is less than a half g acceleration, if you move a half a foot, which is a relatively small displacement, you cause tremendous damage.

MR. FARMAKIDES: At the surface level?

MR. STOLZMAN: Yes.

MR. FARMAKIDES: Then you think there is a possibility or a probability?

MR. STOLZMAN: I wouldn't venture to guess. One other point I would like to make is it is very uncertain in correlating Richter magnitudes with accelerations and g values. What I have read in the literature, no one will really say that we

do have a correlation.

MR. FARMAKIDES: Thank you very much, sir, you've been very helpful.

Let's take a 10 minute recess.

MR. BALDWIN: I'd like to ask one question before the recess. For Mr. DuVal and Mr. Scheimer, in light of the announced intention to do a massive new discovery of the seismic hazard here, do they intend to continue operations with plutonium in Building 332 while they are finding out just how bad the situation is?

MR. DU VAL: We have no reason to not continue operations based on the information we have now. But we also feel that we have the responsibility to gather more information.

MR. FARMAKIDES: Thank you, sir. Let's take a 10 minute recess.

(A brief recess.)

MR. FARMAKIDES: May we proceed, please. We've tried very hard today to adjust to schedules of some people who have to leave early. We have three people who have been here now since early this morning, and apparently they want to talk before too much longer. And we've asked Mr. Baldwin if in fact he would permit these people to talk, and they say they would do so within five minutes. Could you identify yourselves for the record and proceed? Can you come up here and sit down? Mr. Baldwin, would you excuse them for just a minute.

STATEMENT OF DIANE HUGHES
CITIZENS FOR TOTAL ENERGY

MS. HUGHES: Thanks very much. That was very gracious of both you and Mr. Baldwin. We appreciate it very much, because we do have to leave.

MR. FARMAKIDES: Could you identify yourself?

MS. HUGHES: Yes. My name is Diane Hughes. I am a member of Citizens for Total Energy, a grassroots pro-energy advocacy educational organization based in San Jose. And I live way out in the far south end, and so it takes me a while to get home. I am a member of the public not yet represented at this meeting. Certainly this public hearing service hearing shows democracy in action. But those who have spoken for the public this morning do not speak for me or my organization or my friends or neighbors.

Yes, we are all definitely, definitely concerned. But

we are not panic-stricken. The intensity of the attack on the Draft Environmental Impact Survey, Livermore Lab and this Board leads me to believe that the speakers heard from so far do not present a balanced view or an unbiases opinion, nor are they staying on the subject.

For example, I was disappointed to find that Daniel Ellsberg with his entourage and press corps -- taking up one half-hour of our time at taxpayers' expense to deliver a dissertation on nuclear weapons. It is indeed unfortunate that we ordinary citizens are not given such extensive news coverage and attention. As you see, the press corps has gone. Guess who we're going to see on TV news tonight?

I, too, am very concerned about our health, safety, my life and my environment. Make no mistake about that. But who here is really credible? The questioners we have heard all day? Why should I believe them? What is their expertise, the basis for their disbelief, the cause of their fears? Aren't they too self-styled elitists trying to tell us what is safe and acceptable?

In my opinion, these witnesses who have testified are overly critical of the Draft report, the Livermore Lab, and this Board. And a reasonable balance should be struck. And that's all I have to say. Thank you so much for your time.

MR. FARMAKIDES: Thank you, ma'am. Could you identify yourself?

STATEMENT BY HELEN HUBBARD

MS. HUBBARD: My name is Helen Hubbard. My address is 3401 Little Valley Road, Senola, California. I have lived in this valley for almost 20 years, and I am honored to be speaking before you. Our home is located one-quarter mile from the Vallecitos Nuclear Center.

I seem to be the only person speaking so far who does live in this valley. I wonder if this is some sort of measure of the concern of the people in this valley. I do not intend to enter into a debate over rems versus millirems, plutoniu, tritium, earthquake faults, high level radioactive waste disposal, or hydrogen bubbles. But what I have to say needs to be said, and if you'll bear with my rattling paper and shaking hands and voice, I'll try to say it.

I woke up this morning at 3:30, something I never do because I'm a very sound sleeper and I cherish every moment I can spend under my nice, warm electric blanket. As I lay there, I tried to analyze what woke me. And I suddenly realized I was frustrated, downright angry, in fact outraged. I also realized that I and the majority of the citizens of this county, this state and the country have been outflanked, outgunned, outmaneuvered, and in this room, certainly outnumbered. I simply did not realize that we were in a war just as real as any where the destruction comes from bullets.

We will not in my view be necessarily put to death by

radiation down the line, but much sooner by economic chaos, economic chaos brought about by fear of the unknown.

It is not difficult to be frightened. It is difficult to be reasonable and logical when you are being barraged on all sides with the horror of something you can't see, you can't smell, you can't touch and you can't taste.

Then there's the bothersome problem that if our income happens to be derived from one of the aforementioned facilities, we are summarily dismissed as second class citizens with little or no forum in which to speak, media-wise. And when we do speak, we are suspect because of affiliation with the nuclear industry in any form. And I don't think that's fair.

Another thing that disturbs me is that the majority of us who want to keep our dishwashers, washers, dryers, toasters, et cetera, are somehow immoral or unethical, that we don't care about our children's health, and all we are interested in are our creature comforts, and what we do to the environment seems to be somehow inconsequential. Nothing could be farther from the truth. The fact is, we care. We care very much.

But how do you tell a hungry child whose father is out of work that he is responsible for the unborn generations? And how do you convince other people in the world, especially in underdeveloped countries, that they will be denied the technology that this country has excelled in because of these same unborn generations? It just doesn't make any sense at all.

What is being attempted here, the shutdown of LLL in particular, Sandia,Vallecitos, Rancho Seco, San Anofre and Diablo Canyon has an effect on every facet of our lives, from nuclear medicine to national defense -- indeed, the electricity that flows into our homes. The tragedy of Three Mile Island is that the hysteria being generated by that accident will help preclude any intelligent weighing of the risks of the nuclear option and all the good things that the splitting of the atom has done for us.

It seems to me that we have created an environmental monster, and now is the time for reasonable, logical people to put him back in perspective. Thanks a lot.

MR. FARMAKIDES: Thank you, ma'am, very much. We have no questions. Thank you very much. Mr. Baldwin?

MR. BALDWIN: Yes. Next I'd like to ask Mr. Jim Caid to speak. He's an engineer from San Francisco.

STATEMENT OF JIM CAID

MR. CAID: The name is C-a-i-d, 1932 Foothill Boulevard, Oakland. I'm a civil and structural engineer in California. And I'd like to make you aware of some more of the uncertainties that structural engineers are faced with. We've heard them from the standpoint of the seismologist, and we'll continue along that line, from the viewpoint of the structural engineer on earthquake-resistent design.

I will begin by reading a short list of about 12 points, and produced by one of the major firms in the San Francisco Bay Area and circulated to other structural engineers with the idea in mind that within the profession we all understand what some of the uncertainties with some of the assumptions that we make are when we do seismic resistant design. There are numerous alternate methods of analysis and procedures which can be followed. All of them involve many assumptions and approximations and generally suffer from the lack of empirical data to confirm the procedures.

Some of these uncertainties can be listed as follows:

- The amplitude and frequency content of earthquake input motion.
- The effects of local geologic and soils condition upon the filtering of earthquake motions.
- The modeling of the interaction and the structure and the soil surrounding structural foundations.
- Methods of representing energy absorption in the foundation material and in the structure itself.
- Modeling of the mass and stiffness properties of the structure.
- Representation of nonlinear effects and the inclusion of stiffness degradation.
- The energy absorption under nonlinear conditions.
- The appropriate load combinations and load factors to

be used.

- The allowable stress and/or deformation criteria to minimize damage to the structure and to the enclosure, finish and service systems.

- The ability to idealize a structure as it is designed and design it as it is idealized; and the ability to idealize the structure as it is constructed, and vice-versa.

These procedures, assumptions and approximations are continually subject to discussion by structural engineers. It is not uncommon for a seismologist to reverse his opinions regarding the seismicity of a particular building site, or for a structural ėngineer to abandon certain designing criteria which were once though to be appropriate. This is understandable, considering that in the context that in the whole of engineering science, the fields of seismology and earthquake resistent design are rather young, and that the current state of the art is usually somewhat behind the demands of society at large.

Seismic design has been seriously developing in professional practice for little more than 20 years. The nuclear field is in a somewhat similar situation. At this state I feel that it would be an irrational decision to construct nuclear facilities in areas of high seismic risk, and that to do so is to accept the possibility and the consequences of failure.

The last portion of the statement was my own opinion.

The list of uncertainties of seismic analysis is commonly known.

MR. FARMAKIDES: Do you have anything else, sir?

MR. CAID: No. That's the end of my statement.

MR. FARMAKIDES: Thank you very much, sir. Is Mr. Rutherford next, Mr. Baldwin?

MR. BALDWIN: Yes.

MR. FARMAKIDES: Mr. Rutherford?

STATEMENT OF JOHN RUTHERFORD

MR. RUTHERFORD: My name is John Rutherford. I am a resident of San Francisco at 1141 Chestnut Street, and I am a property owner in the Livermore Valley. I am president of a consulting engineering firm located in San Francisco. I have been a licensed civil and structural engineer in the state of California since 1958. Our firm, which now numbers 30 employees, including eight licensed civil and structural engineers, specializes in the design and rehabilitation of structures subject to earthquake forces. And the work we do includes soils investigation, foundation design, preparation of geologic hazard reports, and the production of construction documents.

Over the past 28 years we have designed and overseen the construction of projects totaling hundreds of millions of dollars in construction costs, and members of our firm have served as members and chairmen of various professional engineering society committees charged with formulating guidelines

for earthquake resistent design. I have personally participated in projects involving design of blast resistent structures, fallout shelters and provisions in the formulation of foundation and earthquake resistent design code provisions for Bay Side construction at both Foster City and Redwood Shores.

My latest assignment has been to head a team of field geologists, sedimentologists, rock mechanics specialists and hydrologists working with the Egyptian Organization of Antiquities to prepare a detailed geologic map of the Valley of the Kings near Luxor to evaluate the present condition of the 25 or so Pharonic tombs built there some 3500 years ago and issue a geologic hazard report and recommendations for conservation and protection of these tombs.

In the course of preparing this testimony I have visited the Lawrence Livermore Laboratory, examined the structural plans of Building 332, which I understand contains some plutonium, reviewed the Draft Environmental Impact Statement dated September, 1978, read the January 18, 1979 Preliminary Safety Analysis Report, and analyzed several supplementary documents.

Based on this material, my experience in seismic risk evaluation, and my knowledge of the state of the art in both geological fault investigation and earthquake resistant design I'd like to make the following comments:

First, findings of the various geologists who have

examined the site do not appear conclusive. The possibility of surface rupture somewhere on the Lawrence Livermore Laboratory site cannot be ruled out on the basis of the present evidence.

The system of faults in the Livermore Valley has generated at least one major earthquake in 1861 of record capable of causing great damage to buildings and structures.

Third, no building or structure can be designed to safely resist fault rupture.

Fourth, in many cases involving ground shaking resulting from major earthquakes, the greatest property damage and loss of life has occurred from the indirect effects of earthquakes: soil liquefaction, fire, flooding, rupture of water and gas mains, failure of secondary structures such as pipe, electrical and mechanical system supports.

I have not been able to examine thevaults, work stations and other instalations used to handle the plutonium in Building 332, for example. But the effect of fire, flooding and utility line rupture on these structures is probably more significant than the response of the building itself.

Given the uncertainties surrounding the fault locations, the seismic design criteria and the performance of auxiliary structures and services in the event of a natural disaster, material posing a substantial health risk to plant

workers and the surrounding community should, in my opinion, be moved to a geographical location not subject to the earthquake risks threatening the Livermore facility.

That is the end of my statement, but I would like to add just one more comment. The statement has been made by the staff of the Livermore Lab that there will be more geological work. I've been involved in hundreds of these investigations, not as a geologist, but as a person charged with evaluating the material the geologists delivered to us. And all too often, I feel quite insecure about the information -- not because the geologists aren't competent, but often it is not possible to find out, even by trenching or by very sophisticated electromagnetic and geophysical techniques. It's not possible always to find a fault.

If you find out, then you know. But often the results are inconclusive. And I'm afraid that in areas as seismically active as the Livermore Valley is, that such studies when subject to close interpretation may very well be inconclusive.

MR. FARMAKIDES: Thank you, sir. Do you have a question?

DR. BEARD: I'd like more information -- not about the geology, but about structures. Given a vertical acceleration of 5 g and a concommitant horizontal acceleration of 5 g, do I understand you to say it is not possible to design a building to withstand that?

MR. RUTHERFORD: Not for an event which would involve surface rupture. As a structural engineer I would not guarantee you that I could design a structure that would withstand that amount of --

DR. BEARD: But without surface rupture?

MR. RUTHERFORD: Without surface rupture, I would feel much more confident. I think that the major concern that I have here is, will there be surface rupture or will there not be surface rupture.

DR. BEARD: Thank you. Dr. Grose also had a question.

DR. GROSE: One question, Mr. Rutherford: On surface rupture, you mentioned, if I understand you correctly, that there is a potential for surface rupture.

MR. RUTHERFORD: Based on the evidence I've seen, yes.

DR. GROSE: And others have mentioned this, too. Can you tell us what your evidence for the surface rupture potential is?

MR. RUTHERFORD: The fact that there is a very closely spaced network of faults, and I think -- although the geologists don't appear to agree on too many things, they do agree that some of these faults are probably recently active. And when you have a system of faults that closely spaced, I don't think you can say with certainty that there will not be surface ruptures on the site.

DR. GROSE: Thank you.

MR. FARMAKIDES: Did you have any comments on that last point? We would like to explore the seismology aspects of this. Could you continue, sir? Mr. Du Val?

MR. DU VAL: Mr. Chairman, in the interests of a complete record on the subject -- there have been a number of comments made that we have not responded to earlier. Maybe this would be the appropriate time to pick up on them with regard to the earlier studies that have been made by Lawrence Livermore and the Department of Energy, and in particular with regard to the UCRL documents that were referenced earlier.

I would like to ask Don Bernreuter of Lawrence Livermore to give a brief study of the study work and the actual field work that has been done that I think might help to add additional information to this proceeding.

MR. BERNREUTER: My name is Don Bernreuter. That's B-E-R-N-R-E-U-T-E-R. I'm an employee of the Lawrence Livermore Laboratory, and I've been involved in the seismic investigations at Lawrence Livermore Laboratory since about 1971 when we first were asked to go out and assess the seismic safety of Lawrence Livermore Laboratory.

MR. FARMAKIDES: Are you a geologist, sir?

MR. BERNREUTER: I'm a seismic engineer. I worked at the Nevada Test Site on strong ground motion. I worked for the Neuclear Regulatory Commission as a seismologist on loan

to them from 1973 through 1975. And currently I serve as a consultant to the geosciences branch of the Nuclear Regulatory Commission. That's part of my major duties at Lawrence Livermore Laboratory, as well as other duties in the seismic area.

MR. FARMAKIDES: All right, sir.

MR. BERNREUTER: And I'm the co-author of one of the original reports that came out which has been quoted today as showing the Tesla fault going right under the Livermore site. Now, in that report we tried to make it very clear that that was simply a projection from the Department of Water Resources of where the Tesla fault went. They simply drew a line in. And at that time I recommended that we -- that is, Lawrence Livermore Laboratory -- undertake an extensive field investigation to determine if there is any potential for faulting under the Lawrence Livermore Laboratory.

So Lawrence Livermore Laboratory then hired John Bloom and Associates, a very respected engineering and geological firm to go out and conduct the necessary geological and geophysical investigations to try and locate any faults under the laboratory, if there were any. As we explained in our Draft Environmental Impact Statement and also in the staff responses, they used a number of geophysical methods to try to determine whether or not any faults passed through any of the laboratory sites. They couldn't identify any fault passing, really,

underneath or through the Livermore site. There were some indications that two faults passed very near the site -- excuse me. One fault they indicated could conceivably pass through the northeast corner of our site which we have indicated on the map as the Doutherty fault. The other fault was the Tesla strand no. 2. It bent away from the site and passed through the very corner of the laboratory site. Then there were several other possible faults which people have talked about magically talking at the Lab boundary.

These didn't magically stop at the Lab boundary. Our geophysical lines started back in the hills and tried to trace these faults into the Lab. As we started to approach the Laboratory site -- actually back in the hills -- we lost all evidence of these faults. We could not pick them up on any geophysical lines. And therefore, the faults simply either did not exist or died out.

One must remember that in trying to determine whether or not faults exist, when you're covered by alluvium like we are at the Laboratory site, it's rather tenuous. You have to do it by very subtle means. And we took a very conservative approach when we drew the two faults in. There were some indications that they were there, but the indications weren't very strong. We very conservatively drew them in.

After that, after we published that report, the U.S. Geological Survey came out and undertook a rather extensive

survey of the Livermore valley, and in particular, our area, and that came out in the Darrel Heard report. And the Darrel Heard studies indicated that actually another fault that we didn't name on our plot, the Las Positas fault, actually was a major fault through the valley and cut off all the other faults. The reason these other faults tended to die out, as we showed on our geophysical lines, was simply because they were truncated by the Las Positas fault. And this report was finished and published in 1977. Since then there have been extensive investigations of the western end of the valley and the Corona fault has been looked at in conjunction to the Las Positas fault.

We will have to go out one more time and look at our Laboratory site to assure ourselves that there are no possible strands of the Las Positas fault passing anywhere near our site. Our faulting investigation was primarily designed to pick up the standard northeast-southwest trending fault systems. But the Las Positas fault system is trending more east to west. So our investigation really wasn't adequate to pick up a possible spur of the Las Positas fault that might pass through the southeast corner of our Laboratory -- which is some distance from any of our critical facilities, but we want to assure ourselves of whether or not any such faulting does exist. And we also wish to dispose of, finally, whether or not the Doutherty fault does come there or not and whether or not the

Tesla fault does exist or not. Based on Darrel Heard's work, it really looks very strongly like these faults do not exist and that the ground water anomalies that we saw were due to other things.

MR. FARMAKIDES: Mr. Rutherford, did you have any questions, sir, that you'd like to pose to clarify the record.

MR. RUTHERFORD: I'd like to agree that finding a fault in the sort of alluvium that is in the Livermore Valley is extremely difficult. And my past experience has been that if you do happen to find a fault by, say, trenching through alluvium, you've achieved a positive result. You know it's there. But if you don't find it, you don't necessarily know that it's not there. It's quite difficult.

And unfortunately, our knowledge of all these things is really rather recent, and we haven't caught up to the rest of the technologies in this country.

MR. FARMAKIDES: Could we explore this a little bit more? I think, Dr. Grose, you had a couple of questions. This might be an appropriate time to put all this together into the record, if we possibly can.

DR. GROSE: I believe it might be helpful if we could derive some clarification on the relationship of the work that has been done to the work that is being planned. Mr. Scheimer gave us a review of thework that is just starting or soon to start. You mentioned that there is going to be one more crack

made at this problem. And I believe it would be helpful to us if you could explain what approach, in short, you're going to make this next time around to provide us, if possible, with the type of information and some degree of certainty that we seek here.

MR. SCHEIMER: Perhaps I could respond to that.

MR. FARMAKIDES: If you both want to contribute simultaneously, that's all right, too.

MR. SCHEIMER: Let me back up a little bit for the edification, if you will, of people who may not know what people are talking about here in terms of inferences. We all know that the San Andreas fault runs from the southeast up towards the northwest. Most structures in California tend to run that way. So when evidence was found for faulting to the south of the Livermore Valley -- what was called the Tesla Ortagalata fault system by Page -- those faults were rather naturally extended up in a northwestern manner, or dotted in. They were inferred.

Now, that was the state that things were at when the initial investigations were done; as Don has said -- and he's more familiar with this than I, since I'm new to the Laboratory no evidence was found, particularly on the Laboratory site, for faulting in this direction. But since that was the sense of most structure, the geophysical lines, the exploration lines, were run perpendicular to these trends to look for them.

The most recent by Darrel Heard whose geologic map I have here, 1977, shows a feature thatwould have run parallel to these geophysical exploration lines. It's very hard to find something running parallel to what your geologic exploration lines are. Since that's the case, our surface geological investigations and our geophysical exploration techniques that we're talking about are going to be running perpendicular to what they were before.

And there are a few other things that I would -- if you want details of what would be done slightly differently, I feel that it would be nicer to have longer magnetic lines, deeper for offset in the basement. I feel that the spacing of electrodes in the resistivity should be wider to look a little bit deeper for groundwater barriers. These are fairly minor things, but I consider them slight holes in the data that snould be filled in.

Once this information is gained, we have been talking about the possibility of bringing in a geophysical exploration company that does high resolution seismic reflection work to try and pin down whether this feature might be running east-west.

Also, the current accuracy of location of epicenters in the Livermore Valley -- or actually in most of California -- is on the order of plus or minus five kilometers from the U.S. Geological Survey's seismic net. We can only then describe

the seismic activity in Livermore Valley as diffuse. We can't pin down where things are. It would be a great help in determining which faults truncate other faults to be able to accurately locate these events. There is a lot of small activity going on -- and to determine a sense of motion, so if we know the sense of motion on one of these faults we can determine if it indeed truncates another.

The evidence that I have gone out in the field and seen in a few recent weeks -- as I said, I've just gotten to the Laboratory. I've been there about six months. I'm new to this geology. But the evidence that I've seen very recently in that time is consistent with the interpretation of Darrel Heard. And I might point out that the interpretations of both Heard and Bloom and Page and Huey, going back, all tend to tie into one another outside of the area of the Laboratory. The gross structure is indeed the same. We want to pin down this, if you will, microscopic structure. And these are the additional things we're talking about.

I also would like to point out that it's necessary for the Laboratory to make some sort of value judgement as to whom to believe, if you will. It's necessary that we form our own inferences, our own geologic model. We have evidence of Bloom on one side, Heard on the other, or Science Associates, if you will, on the other, for another possible interpretation of theValley's structure. This is why we're going to go out

and look at this ourselves. I hope that has clarified some of the things that we are planning on doing and how they fit into the past.

DR. GROSE: There are many questions in detail, of course, that could be asked. I would like to ask one right now. Do you have any thoughts on the wisdom of trenching across parts of the one square mile site of Lawrence Livermore Laboratory proper?

MR. SCHEIMER: I don't know about the wisdom of it, but it's planned.

DR. GROSE: All right. It's planned. Correct?

MR. SCHEIMER: Yes.

DR. GROSE: One more question, on the Las Positas fault: What are your thoughts on that fault now? Are you going to look into it as you would other faults, for one? Do you believe there is a fault there by that name, as described?

MR. SCHEIMER: As I've said, from what I've seen, the evidence is consistent with that interpretation.

DR. GROSS: With whose interpretation?

MR. SCHEIMER: With the interpretation of the fact that the Las Positas fault does indeed exist.

DR. GROSE: Darrel Heard's.

MR. SCHEIMER: Darrel Heard's interpretation. The evidence is consistent. Until the investigation is finished I would be reluctant to say I believed this, because I would

be tending to prejudice the investigation before it's done. I want to keep an open mind on that.

There is activity in Livermore, as I've said. This denser seismic net which we are talking about will help to pin down the locations much more accurately.

DR. GROSE: You do plan to deply a seismic net?

MR. SCHEIMER: Yes. We are working in cooperation with the Geological Survey to install six additional seismic stations in Livermore Valley -- at least six stations -- three component stations, and participate with them on a data exchange. We'll get their data for their nearest stations andthey'll get ours to increase their knowledge of what's going on here. The average spacing currently of stations in the Livermore Valley is more than ten miles apart. We're talking about stations of an average spacing of five kilometers in a number of triangles, hoping to pin down locations on the order of plus or minus half a kilometer, which would indeed then allow us assign activity to one fault or another.

We're also talking about, once these stations are installed, about going back and calibrating them, as it were. It's sometimes done. You set off explosives to accurately calculate the error at each one of these stations which would allow us then, perhaps to go back and relocate thehistorical data, since we would be calibrating their nearer stations.

DR. GROSS: That's all the questions I have.

MR. FARMAKIDES: Mr. Rutherford, in view of those comments, do you have any suggestions that you think might be profitably suggested at this time?

MR. RUTHERFORD: I think that they're proceeding in the right direction, particularly in getting more of a historical record. I think that the whole art of geologic and seismic investigation is still dependent mostly on historical record. That's the most reliable evidence that we really have of seismic activity. And the longer we live, the more we know.

MR. FARMAKIDES: Thank you very much, sir. Mr. Baldwin?

MR. BALDWIN: Yes, sir. I have one little question for Mr. Bernreuter. I noted that in the report on the site UCRL 51193, which Mr. Bernreuter co-authored, he predicts a maximum acceleration for the site of a .8 g. And we note now that the Building 332 is designed for .5. So I'd like to ask Mr. Bernreuter whether he still believes that a .8 is possible at the site of Building 332.

MR. FARMAKIDES: I think that's a fair question. Mr. Bernreuter?

MR. BERNREUTER: I think probably a .8 g in the Livermore Valley is possible. We chose to use the .5 g based on what we felt to be a measure of the seismic hazard in the valley, taking into account the probability of the earthquake occurring and the probability that we would see this high g

value that we had originally predicted, and the fact that the .8 g in the earlier UCRL was based on on the use of reactor-type criteria where you postulate the maximum credible earthquake that's possible; whereas our facilities are much less hazardous than reactors, and therefore one would want to take into account the probability of occurrence of such events and the probability that even if we did have a magnitude 6.5 earthquake, on the alluvial valley that we have, our analysis that we carried out showed that it was very unlikely that you could have any amplification of ground motion through the soil cover. Our analysis showed that there would be no amplification. In fact, there would probably be deamplification of ground motion.

So taking all that into account the .5 g seems to be a reasonably conservative number for us to use in the evaluation process. This is consistent with design practice used by NRC in specifying the ground motion. It's not meant to represent the peak ground motion reading, but the appropriate design g value.

MR. FARMAKIDES: Mr. Baldwin, did you have another question to follow up on that one?

MR. BALDWIN: Well, no. I think I got the answer I needed. If a .8 is possible in the opinion of Livermore consultants and a .5 is what that building is designed for, and that building is capable of causing -- and this is no joke --

thousands or millions of cases of lung cancer for thousands of years, if there is an earthquake, I think it is an unspeakable disgrace that that building remain in operation for one more day.

My next witness is Mr. Gary Gray.

MR. FARMAKIDES: Did you have a response to that?

MR. BERNREUTER: Yes. If I were of the same opinion that the .5 g was as described by Mr. Baldwin, I would have long since written a number of memos in the Laboratory protesting that value. When we arrived at the g value that we finally used for the Livermore facility, this was done considering all these factors, and all the conservatisms that we put into the design of these structures and the design process.

And I want to repeat once again, the .8 g in the first report represented a number that would be seen by a seismometer, and not necessarily the appropriate number to use in the design and evaluation process. The .5 g that we're talking about here is the number that accounts for all the conservatisms in the structural analysis and such like.

The numbers are not inconsistent. The building is in our opinion, in my opinion, adequately safe.

MR. SCHEIMER: Perhaps if I may insert an illustration here. If you will visualize for a moment what happens when the ground moves, a very quick peak of acceleration for a very shrot time, a second or less, is the sort of number we're talking

about here when we say .8 -- 2, 3. A very quick spike,if you will. What Don is talking about is, this spike is very quick. What you want to design your building for is the shaking. So you scale the whole spectrum, the whole time series to some value. And there is perhaps a difference of opinion as to what number you're talking about and which part of the signal.

MR. FARMAKIDES: Could we go back to what Mr. Baldwin said, however? I'd like to know if there is an inconsistency and exactly where the focus of that inconsistency is.
Mr. Baldwin points out that there is some opinion that talks in terms of a .8, and there's other opinion that talks in terms of a .5. And he suggests that that is inconsistent. It appears to me that perhaps it is inconsistent. Now, how do you explain away the two numbers that were used? I think that's Mr. Baldwin's point. Is there an inconsistency?

MR. SCHEIMER: In the illustration that I just used, I was trying to explain that in what is being discussed here, there is not necessarily an inconsistency.

MR. FARMAKIDES: Did you say there is not?

MR. SCHEIMER: Not necessarily an inconsistency.

MR. FARMAKIDES: Not.

MR. SCHEIMER: If you're talking about how you scale -- Don talks about scaling a response spectrum. That's sort of the total building response over time and over various frequencies. One short spike of high acceleration does not

a crushed building make. What you want to worry about is the sort of shaking you design your building for. And what Don has been saying is they scaled their response spectrum to this .5 g value. He was talking about in the earlier report the possibility of a peak acceleration observed at the ground, a spike at .8 g.

As I understand what the structural people tell me, this is the difference between these two numbers.

MR. FARMAKIDES: Mr. Baldwin, did you want to follow up on that, or did you --

MR. BALDWIN: Yes. I believe that a study of the seismologic records that are being developed in these various studies will show that in each case they're talking about a peak. John Bloom's .5 represents an estimate of the maximum acceleration which will be felt during an earthquake at Building 332. Mr. Bernreuter's .8 is an estimate of the maximum, the peak acceleration that will be felt during an earthquake at Building 332. They are comparable numbers.

Bloom thinks that there is less possibility of an earthquake around here than Mr. Bernreuter thinks. And they designed for Bloom. They designed for that lesser possibility. And that building is operating out there right now with 100 pounds of plutonium metal under that seismic assumption.

MR. FARMAKIDES: All right, sir.

DR. GROSE: One comment. I believe that the EIS does

not come through clearly on this point. I believe I understand what you're saying. And I believe the point is worthyof clarification. There have been several comments made here today on confusion and conflict and misunderstanding of what this means. So I would suggest that we make a note of that. That's a very important point here, in my opinion.

MR. DU VAL: Mr. Chairman, we'll have the opportunity in the final EIS writeup to address that and to ensure that these comments are considered in sharpening up and elaborating on that point.

MR. FARMAKIDES: As we said earlier, we have reviewed the Draft Environmental Impact Statement and the staff supplement, and this is one of the issues that we had focused on before coming to the hearing as one that we though required a clarification. And now, in view of the testimony of the various people we've had today, why, it appears that this is an area that has to be clarified further.

Mr. Baldwin, could you proceed, sir?

MR. BALDWIN: Surely. I just wanted to ask the Board a question. Do you believe you have the authority to make a recommendation as to the continued operations in that building while these investigations are going on?

MR. FARMAKIDES: Sir, I think as we've said time and time again throughout the day, our function, our authority is to identify issues. That is the extent of our function. We

identify issues. We can suggest options. But we do not resolve the issues. And that is a categorical limitation on this authority.

MR. BALDWIN: All right. Thank you, Mr. Chairman. Next we should hear from Mr. Gary Gray. Mr. Gray is a structural engineer from Berkeley.

MR. FARMAKIDES: I meant to thank Mr. Rutherford. I'm sorry that he left before I had the opportunity. And also, Mr. Bernreuter, thank you very much, and the other gentleman that spoke. Go ahead, sir.

STATEMENT BY RALPH GRAY

My name is Ralph Gray. Gary is a nickname. I live at 1001 Merced Street, Berkeley. I am here as an individual, not as a spokesman, for the American Institute of Architects, the American Society of Civil Engineers, Consulting Engineers Association of California, and the Structural Engineers Association of Northern California, though I am indeed a member of those organizations.

The first building I designed that got built was in 1949. I was fortunate to have an uncle who could build a building that I sketched. I went to work after school when I got out of graduate school for John Lyon Reed in 1957. We were designing public schools in those days. Aseismic, antiseismic design has been central to everything I've done since then.

When I got out of school I thought I knew all kinds of gee-whiz things about how it ought to be done. As the years have gone by, I have achieved lower levels of self-confidence. It seems that every time there's a major earthquake we learn something. I remember one of the principal designers of the State Highway Department saying that as he displayed -- I have nothing against the man. He's a good man and I enjoy a friendship with him. But he did say, "He sure learned something from that." And that was an overpass that came down.

We seem to keep learning from major earthquakes. As soon as I don't learn something from a major earthquake, I'll feel a whole lot more confident about facing the one that follows.

There are a lot of buildings around here that have my name on them. They don't have the geologist's name on them. They have my name on them. And if something goes wrong, they look for me. We have very little experience with what I might call plutonium-oriented disasters or collapses. We have some experience with buildings of a more conventional sort, such as a public school. But we have no experience with a building that suffers damage that is supposed to stay essentially air-tight, if I understand information from other people.

There are some mundane aspects to construction that I don't think have been mentioned -- for example, shrinkage cracks, places where a building is probably going to crack more

if it is subjected to stronger distress, such as site rupture under the building or nearby. We have developed very quickly, I think, a lot of information that is very useful. I have profound respect for the people that have done it.

But for example, the measures of the plots of intensity or not intensity, let's say severity. That has no precise physical definition. Say, the plots of severity versus distance -- speaking of ground shaking, not rupture. But ground shanking, those plots poop out at one kilometer. We're talking about disturbance much closer, if I understand the seismologists. I'm not a seismologist, but I'm hearing about closer disturbances.

I realize the phrase "surface rupture" has been used frequently here. It is a real concern. I saw a school at San Fernando where a surface rupture passed directly underneath it, and the building was essentially undamaged. If I had seen the drawings of that building, I could not have said it would or would not withstand surface rupture. It was designed by engineers for whom I have great respect. They have checked some of my designs. And the things about that building that helped it to ride out the quake are not things I'm hearing about today. It was a minor displacement, let me add.

They are things such as, how is the building tied together. That's mundane. Where the rebar lapped, in fact. There are a million similar circumstances to the construction

of a building. The construction of a building can be tracked through six steps. One is the perceived need. Do we really need the ting? Two, to find a site. Is the site appropriate? Is it safe enough for the hazard involved.

Third is to define a program. So far, there has been no design professional involved in most projects. At the fourth step, the production of the contract documents, we see the participation of a registered designer in most cases. The fifth step is construction. So far, there have been ample opportunities for mistakes, most of which are not recorded or not even known. This has happened to me. I have had public buildings for which I'm the designer for which there were state or city or federal inspectors where mistakes were made. And there's no record in most cases.

The construction phase is the one I think that's most obvious. It's just part of the system. Mistakes are made and you do the best you can.

And the sixth, of course, is the maintenance and re-modeling, over which at this point usually there is no parti-cipation of a design professional. It's the day to day use of the building, particularly an industrial building. Changes are made by the fellow that has to hang a weight, or what have you. Again, there is probably no participation of a design profes-sional who knows intimately the assumptions under which the building was first designed.

All of the important influences involved in design and construction of buildings change very fast. It's hard to design for a building to be built five or six years hence. I think the nuclear industry is in a trap there, because construction techniques move so fast. For example, the use of slip forming techniques on a doubly curved surface -- 15 years ago I would have been astonished to think that you could slip form a surface like that. And yet people are doing it, as you know.

I'm going to skip through my notes, because others have covered many of the points that I was planning to make.

I, as I hope a responsible professional designer, could not put my name on any building I know of and say, and guarantee in effect that there will be no cracks, no collapse, no puff of interior air due to some accident, perahps, and no battering between adjacent buildings if there is surface rupture in the near vicinity or under those buildings. It is no reflection on the designers to say that that building was designed in 1958. That was before Anchorage. We learned a great deal from Anchorage. We will probably learn more. If we learn anything from the past, it's that we'll probably learn more from the future quakes.

I cannot make any guarantee about any building I ever design about surface rupture. If I did, I'd be in deep trouble with my insurance company.

I have a recommendation to make. I think that in cases like this involving what is perceived by the public to be a high hazard, there should be a review by a firm large enough to handle the job. I think that it would probably be an association of different people. It should be run by a structural engineer, because they're closest to the overall scene and yet are technically oriented. They should select the various geological input sources and so on. But it should be a firm or a group of people that are perceived by the public as being totally disinterested.

I of course could not accept such a commission myself, because I've abdicated it, and I understand that Mr. Rutherford would feel the same way. This is a serious proposal, gentlemen, and I think that it ought to be done for everyone's sake. Thank you.

MR. FARMAKIDES: Thank you, sir.

DR. BEARD: I have one or two questions, one I wanted to ask Mr. Rutherford. No, I'll ask you. You're a structural engineer. I now perceive we've got two things closely related. We've got the earthquake problem, which is one thing, and then we have the structural problem designing for that type of thing. And I find that maybe or maybe not you speak the same language and I don't, so I'm asking for clarification -- leaving surface ruptures out, Mr. Rutherford said that in his opinion, professional opinion as a design engineer, that

the hazards came largely from fire and flooding. Now, these are not, if I visualize my basic physics on structures -- these are not primarily related to the peak acceleration, but related to the shake, which is something quite a lot lower. Am I right or wrong?

MR. GRAY: I think you're right, sir, and I wish I had mentioned that again, to reiterate it. I am not impressed with the use of a peak acceleration. Henry Dagencolp (?) has a marvelous example where he shows that the same stresses are developed in the same column with the peak acceleration of .1 g and I think 10 g. It's a function of the displacement of the column and the load on the column.

Certainly there's some sort of index involved here. Peak acceleration has something to do with a big quake. But I'm also interested in duration. A series of pulses of a relatively small peak acceleration that goes on for a long time can be a very serious event.

The second part, I think, of your inquiry, has to do with subsidiary systems. That's certainly true. The communication between the various designers is sometimes faulty. It's my impression that structural engineers are haunted people, perhaps -- fundamentally optomists, but they try hard to compensate.

My feeling about some of the piping design is that maybe sometimes we don't quite get together. Again, it's not

a reflection. It's just the way people are trained and the history of the professions.

I can see a building that is not housing plutonium but was not designed for severe quaking nevertheless suffering damage in these astonishingly high levels -- I mean 2 g's, a petrifying number. It would suffer some damage, certainly. Any building would -- that interfered with other operations that are necessary to perhaps deal with a minor problem in the plutonium building. There's a bunch of rubble in the street. You can't get to the building. That's not exactly a failure, but it sure is an inconvenience -- and fire, and things like that.

DR. BEARD: I have one more question, perhaps trivial: Is the cost related to a so-called maximum acceleration or, let's say shake, that does this? Is that exponentially related to how much you pay to design the structure, or is it linear?

MR. GRAY: Sir, I have no idea. I'm sorry.

DR. BEARD: If you design for twice, you can't say it's going to cost twice as much.

MR. GRAY: You certainly can't say that. It might. But I would not try to predict.

MR. FARMAKIDES: Thank you very much, sir. Mr. Baldwin, did you have anyone else?

MR. BALDWIN: Yes. I have one other person to call.

I have one little comment for Mr. Beard. The subsidiary failure that we fear at Building 332 is an air leak. They have to design that building to be airtight. Now, if the air gets in there and it gets to the plutonium the plutonium is going to catch fire. And if it's difficult to design a building against major structural damage or to guarantee against major structural damage, how hard must it be to guarantee that a building will remain airtight? Very difficult.

One other gentleman we should hear from -- Mr. Glenn Barlow. Mr. Barlow spoke this morning on behalf of other parties. He is a geologic researcher at Friends of the Earth, and he would like to relate to the Board the testimony of two experts who have written to the Board but are not here today. These are Dr. James Brune from the University of California at San Diego and Pat Griffin, a geologic and seismologic engineer from U.C. Berkeley. Each of these gentlemen has written to the Board, and Mr. Barlow has those communications.

MR. FARMAKIDES: Could we just simply accept them into the record? How many pages are there?

MR. BALDWIN: I think they are about two pages each. We don't intend to read them in full. We just intend to say what they have to say.

MR. FARMAKIDES: And then present them?

MR. BALDWIN: Yes.

MR. FARMAKIDES: How much more time do you need,

Mr. Baldwin? I think that you've pretty well exhausted your time.

MR. BALDWIN: Yes. I think five or ten minutes ought to do it.

MR. FARMAKIDES: All right. Mr. Barlow?

STATEMENT OF GLENN BARLOW

MR. BARLOW: I have here two statements which are being submitted to the Board, along with supporting documents. First is from Dr. James Brune, who is a professor of geophysics at U.C. San Diego with the Insititue of Geophysics and Planetary Physics at Scripps Institute. And he relates that there is available data and physical understanding indicating that accelerations of greater than 2 g are possible, and accelerations of greater than 1 g may be common. I think this is particularly pertinent here in looking at the Livermore site and discussing potential ground motion.

He goes on to say -- and I'm summarizing -- one aspect of the problem discussed in some detail and which may be of crucial importance to the Livermore site is the phenomena of directivity focusing of energy in the direction of fault propagation. Rupture along the Tesla fault as well as long other mapped faults in the Livermore region in the direction of the Livermore Lab site could result in anamolously high accelerations in excess of 2 g.

It is not possible to accurately assess the probability of such an anamalously high accleration, but the effect is well established and commonly observed in rupture propagation. And he refers here to other documents that are being submitted with this testimony that can be analyzed by experts from the Lab and DOE.

Also of particular importance to the Livermore site is the conclusion that acclerations of greater than 1 g will probably be recorded for even low magnitudes. On April 6, 1977 a magnitude of 5.5 shallow earthquake in Iran generated peak accelerations of .95 g horizontal and 1.08 g vertical components, respectively.

Another part of the testimony which is of critical importance to the Livermore site is the reported results from the Baja California earthquake swarm of March 1978 referring to other supporting documents. One event of magnitude 4.9 produced accelerations of about .64 g at a distance of ten kilometers. Although final information on the depth location and mechanism of event are not available, it nevertheless shows that even relatively small events can generate accelerations of over .6 g in an environment of very thick alluvium, such as we have at the Livermore site.

This result indicates that the acceleration value of .5 g for the Livermore site is not conservative.

The next testimony is from --

MR. FARMAKIDES: Is tha dated April the 6th?

MR. BARLOW: That's correct.

MR. FARMAKIDES: So this is the package that I received yesterday, Mr. Baldwin?

MR. BALDWIN: It may be.

MR. BARLOW: Yes, it is. And you have the supporting documents there. I can give you an additional copy, if you like.

The next statement concerns surface rupture at the Livermore site. And it's from Patrick Griffin who is a geotechnical engineer completing his PhD in earthquake engineering at Berkeley. He's been working in the area of earthquake engineering since 1970.

MR. FARMAKIDES: What's the date of that, please?

MR. BARLOW: April 6th also, but you do not have a copy of this.

"In the seismological evaluation section of the DEIS, some care is taken to predict a safe shutdown earthquake for the site. For convenience the author separates the site responses to earthquakes into two categories: large distant earthquakes and surface rupture on nearby faults. In calculating an approximation of the magnitude and acclerations from nearby earthquakes, a procedure is used involving the estimation of fault rupture length of nearby faults and correlating these with recorded rupture length, earthquake magnitude, and

acceleration data. This procedure is often used to obtain a rough approximation of earthquake characteristics.

"However, in this particular case, fault rupture lengths are particularly difficult to approximate because the actual fault rupture is beneath deep alluvial deposits in the Livermore Valley. Surface expressions of faulting may have little relation to true bedrock fault activity." And this is a very important point, considering the proposal for trenching in the Livermore Valley on the site.

It is possible that the faults exist there in the bedrock beneath the Laboratory buildings, but that the surface expressions will not be visible because of thethick alluvium.

Even more serious, however -- to continue this -- is the fact that the various branch faults near the Livermore Lab site are all part of the extremely active San Andreas fault system. Irrespective of recorded earthquake activity in the immediate Livermore Lab area, any of these faults could experience a major earthquake. This possibility appears to escape the attention of the author of the appendix in the DEIS.

When discussing the response of the site to large distant earthquakes, the author chose a scaled version of the record, but made no effort to relate the subsurface strata in relation to the Livermore site. Different bedrock characteristics can significantly influence the frequency and attenuation characteristics along the path of elastic waves.

Furthermore, the selected duration of strong ground motion at the Lab site was less than ten seconds in the DEIS report, and this seems unconservatively short, considering the San Francisco earthquake of 1906 which had a duration of approximately 60 seconds. The author proceeds to evaluate ground surface response by using a lumped mass analysis, using equivalent linear soil properties. This particular type of analysis approximates ground surface response through soil layers of vertically propagating sheer waves.

Among other things, the analysis assumes that the soil is horizontally bedded. And then he goes into a discussion of the soil characteristics. And I won't read the whole thing, but I think that it's very important that this be analyzed, because the way the soil is beneath the lab site with an anticline and incline, it is possible that there could be different kinds of amplification of accelerations in the ground there and that would change the conclusions that were reached in the DEIS appendix.

Clearly, to adequately predict the ground surface response to any bedrock shaking at the Livermore sit, it will be necessary to conduct a more detailed subsurface investigation and conduct a more contemporary analysis of the site than the lumped mass method.

Then he discusses the fact that there is the potential for liquefaction at the site, although Mr. Tocarts seemed to

say in an earlier study that there was no hazard of liquefaction. Griffin would like to note that the fine grained alluvial sands which appear to be characteristic of the lab site can be highly susceptible to liquefaction if they are loosely deposited and if they are in a high water table environment. This is particularly true if the site is subjected to a long duration, more than 30 seconds of seismic shaking.

He therefore suggests that the possibility of soil liquefaction is not to be dismissed lightly.

The last comment concerns the author's treatment of the site response during an earthquake on a nearby fault. And this is the surface rupture section. As he states on page 2 A 26 of the Appendix from the DEIS, "The distinguishing characteristics of earthquakes in nearby faults is the effect of surface rupture." What he does not state in the DEIS is that since the site is underlain by deep alluvium the surface expression of faulting -- i.e., the actual ground surface rupture, may appear anywhere on the lab site, not necessarily above the bedrock faulting.

Although there is very little data concerning accelerations in the immediate vicinity of a ground surface rupture, the author noted that there are indications that the accelerations near surface ruptures are significantly higher than attenuation curves would indicate. It would appear that any critical structures on the Livermore Lab site should be

designed to withstand not only the safe shutdown earthquake shaking, but also significant surface rupture and associated higher accelerations.

Now, I have two comments to make on that. In some of the maps discussing the Tesla fault, no. 1, and the Coral Hollow fault, particularly the map that was prepared by Mr. Bernreuter, which is in an earlier report dated 1972, it shows the Tesla fault and the Coral Hollow fault going through the site beneath the buildings on the site.

And in the DEIS report -- and it refers to John Bloom also in his 1972 report, they all three assume that the Tesla fault continues across the Livermore Valley. And in the discussion of the regional techtonic framework in the DEIS it says that that the Tesla fault is part of the Ortigalata fault system, which is a remnant of the ancient Costra range fault system which extends quite a ways to the south, and apparently extends across the Livermore Valley and continues north into the Stony Creek fault zone.

Now, if this is true, and if it is a possibility which is something that should be investigated in these proposals, then the length of the Tesla fault could be much longer than has been discussed so far. This would give you a potential magnitude much higher than the 6.5 and could give you ground accelerations much higher than .5 g or .8 g. And I believe that the investigation should look much further south and much

to see if the Tesla fault does continue south and north, connecting with these other two fault systems that have been indicated in all three reports published by the Lab.

Briefly, I would like to say that in the Darrel Heard report it's noted thatin 1943 there was a swarm of about 18 earthquakes in the Livermore Valley and the epicenters were throughout the central part of the valley north of the Las Positas fault. And when the discussion of truncation took place, and the fact that you're now proposing to put instruments in the valley to detect the epicenters, you might look at these maps of the swarms of earlier earthquakes, some of which were above a 5 on the Richter scale, and they were north of the Las Positas fault, which seems to indicate that the Las Positas, if it exists, does not truncate the Tesla and the other faults that trend parallel and normal to the San Andreas and Calaveras systems.

Also in John Bloom's 1978 report, he notes that on June 21st, 1977 there was a moderate earthquake on the Tessla fault.

One final point is I have here a document which you have there which I'm not going to read at all from, but I think it needs to be considered seriously. It's from a Dr. Michael Trifunac, who is a professor of earthquake engineering at the University of Southern California. And in here, just the title of this document is, "Preliminary Analysis of the Peaks of

Strong Earthquake Ground Motion, Dependence of these Peaks on Earthquake Magnitude, Epicentral Distance, and Recording Site Conditions." And it was shown to me by Dr. Brune that there is a table in here that I've pointed out to you in the letter that you have there -- from which you can calculate for a 6.5 magnitude earthquake in alluvium soil with 90 percent confidence, you can, according to Dr. Trifunac, at a zero distance -- meaning if the epicenter of an earthquake was on the Tessla fault at the Lab site at zero distance, you could get in excess of a 3.0 g peak ground acceleration.

MR. FARMAKIDES: I think we have that, sir. And I think this issue we have developed to the point now where it's rather clear that this will be one of the issues we're going to discuss.

Did you have anything else, Mr. Baldwin? I think we're up to now three hours. And you had requested two hours, sir.

MR. BARLOW: No. We started after 3:00.

MR. BALDWIN: Actually, we're up to about two. But we are through.

MR. BARLOW: I just want to make one final point. Earlier, you were discussing .5 g and .8 g as the parameters that you were considering. But I have presented here testimony from experts that require you to consider 1.0 g, in excess of 2.0 g, and in excess of 3.0 g. And I would like to note that at the Vallecitos Nuclear Center, the Nuclear Regulatory

Commission recently concluded with their consultants and with the USGS that Vallecitos, which is only eight miles from here, you can get in excess of a 1.0 g. So I think you've got to look at in excess of 1.0 g. You've got to consider the possibility of in excess of 2.0 g and 3.0 g when you consider earthquakes at the Lab site.

MR. FARMAKIDES: Okay. We have a question.

DR. BEARD: I have a question for Mr. Barlow. Just for the completeness of the record and without attempting on the scenarious or anything, but as I understand now, your major concern is against the plutonium building?

MR. BALDWIN: We believe that that's the biggest hazard out here. There are a lot of others, but that one's number one.

DR. BEARD: But that is the major one right now, is the plutonium building?

MR. BALDWIN: Right. 332.

MR. FARMAKIDES: Mr. Du Val, you had something to say, sir?

MR. DU VAL: Mr. Chairman, I would request a few minutes briefly for Mr. Frank Tocars, who has been referenced here as one of the authors of an earlier work, to be able to make a few comments.

MR. FARMAKIDES: I think that's fair, too. Go ahead, sir.

MR. TOCARS: I'm Frank Tocars, and I have a PhD. My

major is structural engineering. I have had 19 years of experience in the structural area. The last nine years have been in the seismic area. I have a staff at the Livermore Laboratory of some 60 people. The majority of them are in the seismic area. I've heard a lot of discussion about uncertainties. Out staff is aware of these uncertainties. I'm also the program leader for a $12 million program with NRC that has its goal to try to tighten up the band on uncertainties in the seismic type of methodology.

I'd like to just make a couple of comments. As an author with Don on this report that's being referred to, I'm a structural type, and in hindsight I would have removed from the title the word "design," and reflected that our objective was to have a peak ground acceleration at the Livermore site. And the .8 number that came out of the report, it reflects a possibility of .8 g at the site.

As a structural engineer, I would prefer for people to talk about an effective g level. And that's a ground motion that could cause structural damage. That's a hard thing to do. I can go out on a site and I can make a measurement for peak g level, and I can get numbers like I've heard here today. When I talk about damage and what g level causes damage, I have to go around and observe damage after earthquakes.

I do know that it's hard to come up with an effective number, but the number is always lower than the peak number

that people quote as what they have measured at the site. The effective number is that number that would cause damage to structures located there.

I'd like to make a second comment with regard to a design of .5 g on the plutonium building. I guess first I'll back off and I'll say that in designing buildings to .5 of a g, that level of effective g in design is practically greater than all design structures in the country. There are very few structures designed at ground motion levels greater than .5 of a g. Maybe the exceptions are they're evaluating the Diablo Canyon Power plant at .75 g's and the San Anoffre power plants. But in general, structures are usually designed in the range of an effective g of .1 g and .2 g's, in that area.

I would like to make a comment about Building 332. It is being designed for .5 g's. We have made a conservative evaluation, and we feel that there will be no substantial structural damage at .8 g's. That's theend of my comments.

MR. FARMAKIDES: Did you have anything else, Mr. Baldwin or Mr. Barlow?

MR. BALDWIN: One other little thing. There are a lot of recommendations about doing studies, and I think that those are all good recommendations. But this is a desparate situation. It's five minutes to 5:00, and the earthquake could come before 5:00. It may come tomorrow, and it may never come. But if it does come, if there is an earthquake out

there, there is 100 pounds of inflammable plutonium metal in that building. And I've said it before, and I guess I'll say it again: It will give you cancer if you inhale a billionth of a gram. There's enough plutonium out there to give cancer to everybody a hundred or a thousand times.

And it would be a disaster far beyond anyone's imagination if that building is damaged and air gets inside and you have a plutonium fire and the plutonium oxide gets out.

I guess that's about all we have. One more thing, I guess, from Mr. Barlow.

MR. BARLOW: I have two questions of Mr. Tocarts, since I've been reading his report so much, and based on what he just said. I wouldlike to ask him, first of all, was the building in which the plutonium -- the plutonium metallurgy building has had increments added recently. But I understand that the original plutonium metallurgy building, the larger portion of it, was built in 1958. Is that correct?

MR. FARMAKIDES: I think we'll have that question answered. Can you answer it, sir?

MR. TOCARTS: I can't answer that question as to what time it was built. I can't recall right now.

MR. BARLOW: Okay. My real question is, is the .5 g that's being used now for designs for new buildings -- but it's my understanding that the .5 g is for structural modifications to older buildings, the implication being that

the buildings built in the 1950's and the 1960's were not built for .5 g, they were built for what you indicated as normal, .1 or .2 g, and that after the1972 reports by yourself and Mr. Bloom, that structural modifications were proposed to bring these buildings up from a .2 g to a .5 g. Is that correct?

MR. TOCARTS: I'm sorry. I didn't hear that.

MR. FARMAKIDES: Could you restate that, please?

MR. BARLOW: Is it correct that after the studies done in 1972 by yourself and Mr. Bloom in which you arrived at a .5 g value for designs of future buildings, that then it was proposed for structural modifications to the older buildings to bring them up to a .5 g, but according to what you said earlier they may have been designed for a .1 or a .2 g.

MR. TOCARTS: Well, there's a difference in design and evaluation. We have evaluated those other increments at .5 g's, and we've evaluated them at .8 of a g, and we do not expect any major structural damage to those increments 1 and 2.

MR. BARLOW: Those are the new buildings?

MR. TOCARTS: That's increments 1 and 2.

MR. BARLOW: What about the older buildings?

MR. TOCARTS: Those are the older buildings.

MR. BARLOW: The plutonium metallurgy building -- was not the original building without theincrements built in the '50's?

MR. TOCARTS: Increments 1 and 2 are the older part

of the plutonium building. Those were evaluated at .5 and .8 g's. And our evaluation shows that there won't be any major structural damage to those buildings at .8 g's.

MR. BARLOW: Have they had structural modifications added since 1972?

MR. DUVAL: There has been an upgrade program in those facilities. It's ongoing now, with regard to some of the ducting and piping in those facilities to bring them up to that level.

MR. TOCARTS: I'm sorry. I was trying to recall between the Building 332 and the other buildings. When we made our evaluations at .5 g's, that was our first go-around. We made recommendations for modifications, and the building was then upgraded and the modifications had been completed. And based on the new modifications, our stance is that the building is adequate to withstand .8 g's. That's increments 1 and 2, the older portion of the building.

Increment 3, the newer portion, is being designed at .5 g's.

MR. BARLOW: Can I have one more question? If you got a peak ground acceleration in excess of 1.0 or 2.0 or 3.0 g's, what kind of effective ground acceleration would you derive from that? Would it not be possible to have the effective acceleration above 1 g?

MR. TOCARTS: I do not have the answer for that. I

don't have any idea.

MR. FARMAKIDES: Did you have something in mind, sir?

MR. BARLOW: Yes. I would propose that the investigations that are being undertaken beginning soon regarding geology and seismology also consider a structural engineering problem in which you could get an effective ground acceleration in excess of 1 g.

MR. FARMAKIDES: Did you intend to do that or something close to that, Mr. Du Val?

MR. DU VAL: Well, my understanding of the ongoing seismic work is that it's going to be an assessment of the facilities from a probabilistic point of view for the range of ground motion that we have concluded, as well as the search for additional information in terms of faulting. I'm not sure whether I fully understand the implications of what the gentleman said, so I don't want to mislead by a simple yes or no.

MR. FARMAKIDES: Could you restate your question, Mr. Barlow, and then let's wind it up, because we've gone over this an awful lot. But go ahead and restate that particular question.

MR. BARLOW: Considering the evidence that we're presenting that you can get 1, 2 or 3 g's in peak ground accelerations, we would like for Mr. Tocarts to use his staff of 60

or whatever to evaluate what would be the effective ground acceleration at the Livermore site if you did have a peak ground acceleration of 1, 2 or 3 g.

MR. FARMAKIDES: We can't evaluate that question. Can you do that, sir?

MR. TOCARTS: As a structural engineer -- and I'm not an expert in the ground motion area. But what I've read over the last five or ten years, from my standpoint, my view is I don't see any basis to even talk about 2 g or 3 g.

MR. FARMAKIDES: Well, Mr. Barlow, can we have your final comments?

MR. BALDWIN: Dr. Brune says 2, Dr. Trifunac says 3, and a lot of people say 1.0. And we would like to have those figures be taken as peak accelerations, and if they want to reduce them to effective values, then they can do that. We want to see what building is going to look like after an earthquake like that. Don't forget. You just need a little crack. That's all.

MR. FARMAKIDES: Thank you very much, sir. I think the next person on the list is Mr. Robert Zatkin. Well, let's take a five-minute recess.

(Off the record.)

MR. FARMAKIDES: Mr. Robert Zatkin?

STATEMENT OF ROBERT ZATKIN

MR. ZATKIN: My name is Robert Zatkin. I was born and raised in the Bay Area. I attended the University of California at Santa Barbara. I majored in biology and geology. I have read this Appendix 2A of the Draft Environmental Statement. And this paper I have in my hand and copies which I've given the Board are my questions and comments about this portion of the DEIS. And I'm just quickly going to read through it, because I found that a lot of the things that I came across, other people have been mentioning.

MR. FARMAKIDES: Well, rather than repeating the same things that were mentioned earlier, sir, could you simply summarize, and then we will include this as your exhibit in the record?

MR. ZATKIN: Okay. Very well. Progressing through this part of the report, my first comment is on the author-title page. I found that there was no indication given as to the professional badkground of the author, and I was left at a loss as to whether he was a geologist or a seismologist or an earth scientist.

So I called the California Division of Mines, and I talked to one of their geologists. And the person that authored this report is neither, according to him, a California professional geologist, he is not a registered

professional geologist, nor is he a member of the Geologic Society of America. Apparently membership in the GSA is pretty much standard fare for professional geologists.

Now, I feel that given the enormous risk of plutonium contamination of the environment in the event of a seismic occurrence in the lab, why was this report not conducted by an independent Government agency such as the USGS which has its western headquarters in Menlo Park or a team of geologists and seismologists from the University of California?

Now, the next question I have is on the introduction on page 1 in the first paragraph. It says that the investigation was performed under the guidelines of a format put forth as per reference number one by the now defunct Atomic Energy Commission. Now, my question is, have the guidelines for this type of investigation been revised under the recently formed Nuclear Regulatory Commission, and have discoveries and advancements in knowledge in geology and particularly in seismology been incorporated into the present design standards and analysis formats for instalations such as the Lawrence Livermore Labs?

The next area that I question in the report is that titled "The Livermore Valley Geology," and particularly the hydrology section on page 13 in the first paragraph. The author states that a high extraction rate has occurred in the water table of the Lawrence Livermore Valley. And I would like to know if, through some mechanism which I won't get into, there

is an escape of plutonium into the environment and it is of a water-borne nature, say hypothetically, a first year's rainstorm with a coincidental earthquake. What would be the consequences of rapid infiltration of plutonium into the ground water table through a water medium?

Another question that came to mind concerns the proximity of the South Bay Aqueduct to the Labs, and what would be the possible consequences on the hydrology of the San Francisco Bay and the people living around it if plutonium were to contaminate this water supply?

Now, on the next section, "Geologic History" on page 14, the first paragraph, the author states, "The occurrence of folding and block thrusting on the western boundary of the Livermore Basin . . . " and at no point in the report is mention made of the Vernona fault located along the western boundary of the basin, nor of the Las Posidas fault, which I believe trends north-west -- well, approximately east-west across the basin. I would like to know why these faults were not considered in the seismic evaluations.

A recent NRC determination on the Vernona fault placed an EPGA, estimated potential ground acceleration, along this fault at or in excess of 1 g and a displacement of three meters, which I believe was confirmed by large scale trenching operations. Why are these values not discussed in the report?

Concerning the site geology and the structure section,

on page 14, the first paragraph, the author states that the Livermore Lab site is on quatrinary alluvium. I'd like to know why a discussion was lacking concerning soil mechanics of alluvium in general, and why no mention was made of anlysis of the alluvium at the site, if in fact it was performed in terms of the particular mechanical behavior of the alluvium at the site, especially as it relates to strong ground motion and rupturing at the site.

In the section titled "site structure" on page 18, first paragraph of that page, the author states the Tesla fault, on which he delineates three strands on figure 9, is the northernmost segment of the Tesla Ortiglita fault system. And he also states thatthe northernmost end of the fault is not known, but it certainly crosses the Livermore Valley.

Now, in figure 9, the trace of the first strand of the Tesla fault ends abruptly at the southeastern border of the site. My question is: Why?

On page 18, the first paragraph, the top right side of the page, the author states the Doutherty fault -- I'm not sure how to pronounce that -- "is a minor structure which could be of significance only in terms of surface faulting." Now, this fault, as I read it with the scale on the map, passes within 200 feet of the Lab site. I'd like to know wny no further consideration is given to possible surface rupturing on the site, the Lab site, from seismic events propagated from

this fault. And figure 9 also shows a possible branch of the fault, the Doutherty fault, which occurs northwest along the trace of the fault. I'd like to know, what of other branches which might be unknown but exist, to the southwest of the fault's trace closer to the Lawrence Livermore site.

In the same section, the caption to figure 10 states that the cross sections AA' and BB' are through the site. And that's pretty much the caption reads, and I assume he means the Lawrence Livermore Lab site.

And if you look at figure 9, the cross section AA' is according to the scale approximately 500 feet to the southeast of the site. And if you look at figure 8, the cross section BB' is adjacent to the southwest border of the site. So why does the author state in the caption that these cross sections pass through the site when in fact as I interpret it, they do not?

Now, in the section titled "Status of Faulting Around the Site" on page 21, concerning the entire page, the author states that "Movement on the Livermore Greenville Rigs Canyon and second strand of the Tesla faults could reasonably be expected to produce movement on the formerly listed faults." And in the next he's referring to as formerly listed the Mocho, the Ramp Thrust, the Coral Hollow, the Doutherty, the Carnegia and the Patternson Pass faults. My question is, why is there no consideration in the report for the large

magnitude seismic event on the San Andreas, the Calaveris or the the Hayward faults could initiate movement on any or all of the faults laced throughout the Livermore Valley?

Concerning Table 4 on page 23, I think that the geologic evidence such as trenching and bore holes and geophysical studies for some of these faults listed in the table as to their length have not been fully utilized, and in particular, as the author states, that the full length of the fault is uncertain. The Tesla and possibly the Coral Hollow fault, the Doutherty fault, the Patternson Pass fault and the Greenville Riggs Canyon fault. So why have not all known means been used to determine the trace and the length of these faults?

Now, the next thing that I would like to comment on is the seismological evaluation, in particular the bedrock shaking section and the peak bedrock accelerations. On page 25 the author presents the record from a 1952 Kern County earthquake as a representative earthquake for bedrock acceleration determinations at the site. My questions are: Why does the author feel the Taft record "adequately captures the bedrock frequencies," even though, by the author's admission, the data was not recorded on the bedrock. The data was recorded at a "range of 42 kilometers." And I assume by "range" he means distance. I was uncertain as to that. Whereas the Calaveras fault is located approximately ten kilometers, ten

kilometers -- ten to fifteen from the Lawrence Livermore site. And I would like to know why there is no consideration of this difference.

I'd like to know how, in a mathematical sense, the author was able to scale down the accelerations to bedrock values in the Tafta data. I'd like to know what type of rock the acclerations were recorded on in the Taft record. On page 25 the author calculates the soil response by other peoples' methods, but he has absolutely no mathematical treatment in the report concerning the soil responses.

On page 25 the author refers to bore holes drilled by Shanan and Wilson, Inc. and Hershey Oil Company as evidence for bedrock depth. Now, no place in the report is the location of these bore holes given. I'd like to know where these holes were drilled. Were they drilled on the Lawrence Livermore site? Another thing I'd like to know concerning the bore hole of Shanan and Wilson, how is it concluded from a 90-degree deep bore hole that bedrock was located at a depth of 400 feet?

My next comment on page 25 -- and this has been gone over. The author presents a bedrock acceleration value of one-half g. Why are the authors' calculations from which the one-half g value was derived not presented in the report? My last concern is the safe shutdown earthquake. The author states that despite his conclusions and those of reference 2

of the report, the Lawrence Livermore management has "concluded that the degree of conservatism contained in this report the DEIS and reference 2 is excessive," and that a lower SSE value is to be "recommended for inclusion in all safety analysis reports for the site is the SSE in reference 2, i.e., Bloom's value."

Okay. What are the reasons for Lawrence Livermore Lab's management deciding that the SSE of the DEIS in reference 2 are conservative? What is the expertise background of those who made the decision to adhere to the SSE of reference 3? And why is it that outside seismologists were not consulted in this decision, especially given considerations of peak acceleration potential as discussed on page 28 of the report.

And that's pretty much the extent of my criticism. I don't want to belabor the point. I think it is a poorly done example of science. And what I have found since I've left the University is that there's a lot of this stuff passing for good science. And I'm pretty concerned because I don't want to have to die because of an accident resulting from a seismic event concerning the plutonium labs.

The last thing I'd like to show the Board, because I think it gives a good visual idea of what's going on in California from a seismic hazard standpoint, is the 1977 map, the latest map of the California Department of Conservation. This is an earthquake map. I'd like you to pay close attention

to the incredible number of faults that occur in this state.

MR. FARMAKIDES: Thank you very much, sir.

MR. ZATKIN: So my concluding comment is that we can pour a lot of money into defining the faults into a lot of mathematical manipulations to define the potential seismic hazard. But I really don't think we can insure in any engineering sense of seismological sense against the hazards that we have. And I think that the most rational thing to do, the thing that strikes me as being -- in terms of my scientific intuition, is to move the laboratory to an area where there is next to no seismic hazard potential.

MR. FARMAKIDES: Thank you, sir. Thank you very much for the work that you've done. I must say one thing, though. I have seen no evidence of any manipulation. Forgive me for stating this point. But I think it's important that from my perspective I see differences of opinion, and I see obvious differences of opinion between experts, scientific experts and other experts. But I see no manipulation.

MR. ZATKIN: Okay. I apologize if I used that word.

MR. FARMAKIDES: It's just a question I -- right.

MR. ZATKIN: I do think that this particular aspect of the Environmental Impact Statement need much further consideration and energy being put into it.

MR. FARMAKIDES: I appreciate your concern, and I very much appreciate your comments. Dr. Grose, you had a

question here.

DR. GROSE: Mr. White, obviously you've done a lot of homework.

MR. ZATKIN: Zatkin. Mr. White was the author of --

DR. GROSS: I'm sorry. Mr. Zatkin. Thank you. Mr. Zatkin, obviously you've done a lot of homework and given a great deal of thought to this report by Mr. White. Have you given the same amount of attention and analysis to several other reports that have been written in this same vein before and since the White report?

MR. ZATKIN: No, I haven't, in all honesty, because I only received a copy of this within the last two weeks. So I was pressured for time. I was very reluctant to appear today because I realized that I didn't have all the information. I was impressed with the 50-odd references made in Mr. White's report. But I just did not have the time to go through them like I would have liked to have done. If I'd known a month ago that this was going to happen I would have been better prepared in that sense.

MR. FARMAKIDES: Do you know what we could do -- and I think it would be very useful. Could we ask the staff to respond to this letter, in addition to the other two letters, and to provide your comments to the questions raised by Mr. Zatkin. Mr. Du Val?

MR. DU VAL: We will give it our best efforts here.

MR. FARMAKIDES: It's an extensive document, no doubt about it. There are a lot of questions, but there are an awful lot of them here that I think have already been answered today. I've seen three or four that have been answered. But it might be worth while to provide you with a response to this. Could you do that, Mr. Du Val?

MR. DU VAL: Yes, sir. We certainly can indicate where that material has already been developed in response to some of these points that Mr. Zatkin is bringing up now and/or where it was developed today that it would be in the testimony.

MR. FARMAKIDES: I think that would be adequate, because it would lead you then to the answers that you're looking for here. I don't know that all of them have been answered, but some of them that I know of here. And I'm not a seismologist or a geologist by any means, but Dr. Grose is.

Let me ask one point, however, Mr. Du Val, with respect to the first question, with respect to the author of theDEIS. Mr. Zatkin, did you mention that there was such a name that was given to you?

MR. ZATKIN: No, there was not. I called the California Division of Mines at the Ferry Building last week and I talked to a geologist and he looked up on the registry of Registered California Professional Geologists. This individual was not listed, nor was he listed as a member of the Geological Society of America.

MR. FARMAKIDES: Who is this individual?

MR. ZATKIN: The geologist at -- I'm sorry. Mr. White is the author of Appendix 2A.

MR. DUVAL: Mr. Chairman, if you'll permit, in the interests of the completion of the record today, I would like to read into the record a few of the features of Mr. White's resume so thatthe Board may know of his background.

MR. FARMAKIDES: Let's do that, just to complete it.

MR. DU VAL: Mr. White has a B.S. degree in engineering from Boston University, an M.S. from Penn State University and graduate studies in geophysics from the University of Washington. During the period 1972 to '76 he was the geotechnical engineer at the Lawrence Livermore Laboratory. He is at present a director of geotechnical engineering at Terra Corporation.

During the time he was at Lawrence Livermore Laboratory he was principal investigation for the NRC soil structure interaction research project and co-investigator for an NRC seismic design base project. And he contributed to other NRC projects, as well as supervise all geology and seismology studies necessary for the preparation of SAR's for the Laboratory's critical facilities.

Prior to his employment at Lawrence Livermore, he was a university instructor, andprior to that he was employed as a mechanical engineer. His professional associations include membership in the American Geophysical Union, membership in the

Seismological Society of America, and membership in the Earthquake Engineering Research Institute.

MR. FARMAKIDES: All right, sir. Thank you. Did you have anything else, Mr. Zatkin?

MR. ZATKIN: Well, I'd like to know, given those facts, why was not some listing of his competency, his background, given in the report?

MR. FARMAKIDES: In which report?

MR. ZATKIN: In his Appendix 2A, "Geologic and Seismological Implications," et cetera. Why simply the man's name? Why not the fact that he has these degrees and his affiliations?

MR. DU VAL: It was a staff report prepared by the staff. And we do not normally identify and give background on the individual staff members.

MR. ZATKIN: Well, he's identified on the front of the report by name. I mean I don't understand this.

MR. FARMAKIDES: Yes. Well, the answer given by Mr. Du Val, whether you agree with it or not, or whether anyone agrees with it or not -- I think from a point of view of most of these reports, these very thick reports, in view of the fact that there are so many people involved in it, generally the name of the individual or several individuals is not given. Whether it should be given or not perhaps might well be a question here.

MR. ZATKIN: His name is given.

MR. FARMAKIDES: In the appendix?

MR. ZATKIN: Right on the front of the report. I assume he authored it.

MR. FARMAKIDES: That was in 1973 or '74?

MR. ZATKIN: '74. I won't belabor this point. I've taken up enough time. I appreciate your listening.

MR. FARMAKIDES: Okay. Thank you. Mr. DuVal, did you have something else?

MR. DU VAL: Just a closing point there in the interests of clarification. That appendix was a separate laboratory document that was incorporated as an appendix and that is the reason for Mr. White's name being on it.

MR. FARMAKIDES: I see.

MR. DU VAL: The balance of the report has a variety of contributors, as would normally be the case. And there are not separate credit lines in each of the sections.

MR. FARMAKIDES: Okay. Thank you. I think we have, then, Ms. Diane Thomas-Glass, Mr. David O'Page -- and that will bring us down then to --

Ms. Glass?

MS. THOMAS-GLASS: Thomas-Glass.

STATEMENT OF DIANE THOMAS-GLASS
ECUMENICAL PEACE INSTITUTE

MS. THOMAS-GLASS: I am speaking as a staff person of the Ecumenical Peace Institute at 944 Market Street,

San Francisco.

The first thing that I want to say is that I'm very, very tired, and I'm sure you are, too. And I'm sure everyone else in the room is. And unfortunately, my feeling about that -- and I want this in the record -- is that there are ways in which this hearing has been very poorly managed. I'm absolutely certain it would have been clearer to have a very strict deadline and have not allowed anyone else to join in and have given people times.

There are people sitting in the room now who have been here with me since 9:00 who still haven't spoken. I understand the dilemma that you face in terms of hearing from people. But for you to have to sit here and listen and the experts over here to have to sit here and listen and the rest of us to have to sit here and wait renders most of us virtually unable to talk at 5:00, after that long of hearing these kind of facts.

I'm sure everybody in the room will be glad to hear that I'm not going to talk about geophysical facts. I'm going to talk about two sections of the DEIS that concern me. One is the section on economic impact and the second is the section on alternatives. Just for the record again, this is Section 1021.41 which reads briefly -- this is the NEPA from 1973. "The DEIS has responsibility for evaluating the long and short-term impact, both direct and indirect, of DOE actions on human physical and social surroundings as well as the

natural environment. That pertains to the economic impact. And secondly, on alternatives, as was pointed out earlier, the DEIS has the responsibility for rigorous exploring, developing, analyzing and implementing of alternative actions, should they be needed.

Under economics -- and my comments are very brief. A woman spoke today about her concern about economic chaos. I'm concerned about the fact that the DEIS does not site the Bureau of Labor Statistics job study which shows that any money spent in the military sector in this country produces far less jobs than spent anywhere else. We are wasting money in terms of unemployment by spending it in the military sector at the Livermore Lab. It certainly affects the Livermore community, as well as the rest of us who don't live in Livermore.

Secondly, recently in California we held state hearings on the impact of military spending in California, and one of the key concerns of those hearings were the boom and bust cycle of military spending where people -- when contracts come in there's employment, and when they don't, there isn't. And a key example of that is the Southern California B-1 bomber experience where people were laid off with a day's notice.

I want to read something from the "Livermore Independent." First of all, an editorial from the "Livermore Independent" which relates to this -- which also relates to the possibility of a comprehensive test ban which could affect

employment at Livermore. And I quote from that editorial: "If test or arms limitation treaties are signed, Livermore Lab may lose considerable funds and may be forced to lay off some of its 6,000 employees. To date, the Lab's only plans for such an eventuality are focused on finding ways to circumvent the treatay so nuclear weapons development may continue. We think the Lab's management's approach is dangerously short-sighted and a clear example of themilitary domination of what is supposed to be a University ofCalifornia-operated research laboratory. The University and LLL administrators should carefully prepare plans for conversion of the Lab to peaceful research, which may give America and the world alternatives to widespread death and destruction."

My concern in this quote is the possibility of massive layoffs with a day or two's notice at Livermore. Nothing in the economic impact part of the Draft Environmental Impact Statement speaks to this point. It's a glowing report of how Livermore has helped build the employment -- the Lab has helped build the employment at Livermore.

Many things which are not appropriate for me to go into today point to the need for conversion plans at Livermore. In 1977 in August, Richard Wagner from Livermore was quoted as saying that conversion of Livermore was an impossibility, that conversion from nuclear weapons work to any other kind of work was an impossibility. And yet today Richard Wagner is

quoted in a local paper as saying that Livermore is always looking for input from other kinds of people, is always open to the possibility of input from other people. I'm interested in the juxtaposition of those two things and wondering how open Livermore really is to possibilities like a comprehensive test ban.

And we at our office have documented the fact from Herb York, who was the first director of Livermore, that every director at Livermore besides Herb York has always testified against the possibility of a comprehensive test ban or a threshold test ban. These kinds of things have direct impact on the kind of economy that our country is based on now -- what has been called at Columbia University a permanent war economy.

It's been documented in studies at Columbia that this economy is depleting the American economy. The woman who spoke about dishwashers and toasters and so on -- I wish I could have spoken to her and encouraged her to read some of these studies and find out why our shipbuilding, why our television sets, why are shoes are no longer competitive in the international market. There is a direct relationship to the kind of dollars that we pour into more and more and more military spending.

It's my opinion that this is part of the economic impact that this study, if it's going to talk about economic impact, should be talking about. It certainly affects the

Livermore community, and it certainly affects the rest of us.

One more thing about the conversion: We've begun an effort last June with no money and only our own will to do it, towards a careful study, a preliminary analysis of the feasibility of converting Livermore in the event of a comprehensive test ban or in the event of a change in national policy. Currently 33 percent of the Lab's work is not weapons-related. And I would be happy to document that for you.

The DOE budget for the next fiscal year includes $5 billion for nuclear weapons work, $3 billion for nuclear power work, and $700 million for all other alternatives.

Representative from New York Oettinger in speaking in 1977 to the hearings of the House Committee on Science and Technology Subcommittee on Fossil and Nuclear Energy Research stated, "I don't know of anything that's come out of these laboratories that's actually gone into use. That is an area in which I think we ought to be greatly concerned. We need to begin to get some of the breakthroughs in technology into use to solve the very crucial problems that we have." He went on to say, "I think you are using the taxpayers' money essentially to cooperate with the large energy companies which already have an excessive stranglehold on our society. You see what comes up from the DOE to us is a lot of excitement and huge dollar proposals to extend the work in these very high capital-intensive high-technology fields.

When it comes to solar, we have to be the instigators, or when it comes to fuel cells, or when it comes to any other alternative technologies.

My concern again with this is stated by an employee who did not want to be quoted by name, who said to us recently when we were interviewing him around this conversion study, that he felt that the real national security question in the next several years, the next decade, was energy.

Yesterday in the "Chronicle" there was an article by Richard Barnett on "Redefining the Myth of National Security" -- both of these things, plus the Office of Science and Technology Policy's February '78 study which states that decades of research and development in basic energy science are desperately needed now, and that the DOE has a critical lack of this kind of research in its laboratories.

This is also supported by a GAO study, Government Office of Accounting study that came out in February of '78 which states that very structure of the DOE labs mitigate against any of this kind of non-nuclear work going on.

Again, I can't detail the whole study. It's going to come out next week, and it's going to be more than 60 pages, and I'm sure nobody wants me to do that here today. But what I want to say with that is that the possibility exists at Livermore for solar energy storage, transportation, wind energy kinds of work to be greatly increased. And all of this

directly impacts both the kinds of things that we've heard about the nuclear hazards or nuclear possibilities here at Livermore, and certainly for the rest of us in terms of the kinds of things which you refuse to let us get into here today about national policy.

I just want to quote from these studies, Government studies, Lab employees, community people who are saying, the possibility and the need exist at Livermore to do something with that facility -- not close it down, but do something other than nuclear weapons work.

I have another quote here that I'd like to almost finish with. I have two more quotes, actually. This is from Barry Commoner's book, "The Poverty of Power." "In sum, we are relying on precisely those sources of energy, fossil fuels and uranium, which with alarming consistency violate the essential requirements of the ecosystem, the production system, and the economic system. Because the present energy sources are nonrenewable and technologically complex, they demand progressively more capital, because the demand for capital grows faster than energy production itself. This vital sector of the production system has lost its capability to regenerate.

"Meanwhile, we are failing to draw upon the one source of energy which is renewable, is not subject to dminishing returns, is technologically simple, is compatible with the environment, and is economically capable of

counteracting the inflationary effects of conventional energy production, the sun."

The final quote -- and I want to share it with you today -- gets at a general concern that I have about these hearings. And again, I've worked with the churches for about ten years now. And throughout those ten years I've had to deal with the fact that a lot of people don't trust organized religion. A lot of people apply to me the kinds of bad experiences or mistrust they have with churches or synagogues. And I feel like you gentlemen have to accept the same kind of guilt by association, because you've accepted a job with a certain institution or organiation or social reality.

And again, it gets back to the first thing that I said about the fact that these hearings are inhumanely run. And I don't have any suggestions off the top of my head today about a better way to do this. I do have a question about what happens beyond here.

You have repeatedly stated that your responsibility is to recognize the problems. And I'm real interested in what happens after that. Who is responsible for answering these questions? I want an answer for thatin just a minute, but one of the reasons I want an aswer for that -- and I can't resist quoting this quote from Harold Agnew, who I'm sure people know is the ex-director of Los Alamos, another DOE laboratory: "The basis of advanced technology is innovation, and nothing is

more stifling to innovation than seeing one's product not used or ruled out of consideration on flimsy premises having to do with public or world opinion."

My concern about that is a deep depression I've been feeling all day about what are we doing here, anyway. What is the purpose of these hearings? Why waste your time? Why waste our time? And I don't have a concluding statement, because I feel like theentire hearings are inconclusive. Can you answer my question about what you do with this information? Who do I write or call or set up a hearing with next week to find out what goes on with this information beyond here?

MR. FARMAKIDES: Ms. Thomas-Glass, I think we've answered it before. We'll do it again. And whether you agree with the system or not is something that I can't control. It's up to you. The system requires -- and this is a procedural system created by the Department of Energy whereby there is an attempt -- and whether you agree with it or not, it is an honest attempt, to obtain an impartial board here to do one thing: to interface with the public, to hear what your concerns are, to articulate the issues, and to detect which of these issues are really unresolved and have to be considered. Beyond that, we have no jurisdiction. We have no authority. Once we render that report, it's a public report. And this is an entire public process.

I frankly think it's a great process. It is an oppor-

tunity for the public to participate. This is public participation in action. And very frankly, I don't know who requested this hearing. I know you all did. But I'm pretty sure that the Livermore people are just as responsible for this hearing as anyone else. I don't know any of these people.

MS. THOMAS-GLASS: My question is --

MR. FARMAKIDES: What happens? We have the report. We'll finish the report. We'll issue it. At that point in time it goes to the assistant secretary of the Department of Energy for Environment, Ms. Ruth Kluson. The decisions with respect to the continued operation of Livermore are the decisions of the Department. The final person that will make any decisions that bear on Livermore is the Secretary.

MS. THOMAS-GLASS: You no doubt know that that's not real reassuring to me. But as you said, that's not your problem.

I want to summarize one more time, then. My concerns and I hope you'll take them to heart -- are the alternatives section of the report, which I consider if we with no money and no staff and nothing but our own will can come up with 60 pages that have to do with job skills and equipment and plant space and factual things at Livermore, I would think that you people could come up with more than a half a paragraph on four things that don't even fill a complete page.

And the second thing is the economic impact section

which I feel is grossly inadequate in terms of looking at the real economic situation that the Livermore Lab puts the Livermore Valley in.

MR. FARMAKIDES: Thank you very much, ma'am.

MR. FARMAKIDES: Mr. Anthony Barreiro?

STATEMENT OF ANTHONY BARREIRO

MR. BARREIRO: My name is Anthony Barreiro. I live at Kresge College, University of California, Santa Cruz.

Before I get into my prepared statement, I read in the EIS -- I think about 11:30 p.m. last night -- on page 3-54, it should be noted that the Livermore valley is not a milk-producing area, and very few milk cows exist within a few kilometers of the site. When I went out for lunch, during the lunch recess today, I bought this little carton of milk from the Holbeiner Dairy in Livermore, California. Apparently, the Holbeiner Dairy has devised a process for producing milk without cows. That's pretty surprising to me.

I'm speaking today for 1100 people at U.C. Santa Cruz, who signed petitions calling for these hearings. These 1100 signatures were gathered from a campus population of about 6,000 by a group of people with little experience and even less money, and in a matter of weeks. People in Santa Cruz care about what goes on at the U.C. Weapons Labs here in Livermore and Los Alamos. We want an end to military insanity and environmental destruction. We are joining with a growing number of people around the world in calling for immediate disarmament and a conversion from centralized capital intesive, ecologically disastrous technologies to those which can be controlled by the people dependent upon them, those which

create work for large numbers of people and which are compatible with the continuance of higher life on this planet.

The Lawrence Livermore and Sandia Livermore laboratories regularly expose their workers and the people of Livermore and beyond to radiation and radioactive isotopes at dosages even greater than those everyone else is exposed to. This exposure is due both to accidents and to routine waste disposal. Much of this background radiation comes from nuclear weapons testing, a sizeable amount of which was American. And we know where these American bombs get designed.

Anyway, I kept reading the DEIS the radiation dosages in the sewers, in the air and in the soil while measureably higher than normal, were all individually within federally established safe limits and would be diluted both in time and space.

I have some serious misgivings about these conclusions that this radiation isn't harmful. I think of thalidomide, DDT, of military personnel being marched under mushroom clouds to see if U.S. troops could occupy A-bombed countries, of those men now dying of leukemia, and of the kind of military double think which led to destroying Vietnamese villages in order to save them. And I wonder what criteria are used to establish safe standards for exposure to radiation.

Could it be that the goals of the U.S. weapons program are of foremost importance, and the lowest exposure level possible,

given these goals, are the safe standards. One must also take into consideration the psychological needs of the people that work with hazardous materials every day and believe that they aren't ruining their health and possibly killing themselves in their everyday work.

The biology of cancer and genetics are still poorly understood and statistical trend to human cancer and birth defects take decades to appear.

By the way, I would like to ask why there isn't a biologist or a physician on the Board today?

MR. FARMAKIDES: It is rather difficult to have the Board composed of all the disciplines that are involved in this particular staff, the staff study. So we have three people who are here not to be necessarily experts in the draft environmental impact statement but to be able to understand what you people are raising as issues.

MR. BARREIRO: How can anyone say what a safe level of exposure is, since we only now are seeing the costs people pay for their safe exposures inthe 1950's.

Right under the DOE's nose in Livermore, Dr. Dobson found attritiated water in extremely low doses causes damage to mouse ovaries and serious infertility. That's been raised today, I realize. But even if the lab stopped blowing tritium into the air and releasing it into the sewers, what safe radionuclide is right now damaging all our health. The research

just hasn't been done on it.

The fact that radio active materials accumulate in the body, and the effects of radiation are cumulative also need to be considered when thinking about safe doses. This seems to be one great inconsistency, shortcoming of the DEIS, in that it only analyzes one isotope in one release and doesn't assess the cumulative effects.

Considering the severity, longevity and slowness to appear, the effects of radiation, it is the height of irresponsibility to legitimate any human to exposure, radiation or radioactive materials.

In a true cost benefit analysis of the Lawrence Livermore and Sandia Livermore labs, the major listed benefit -- bigger and better nuclear weapons is actually a cost. A cost the people of this planet have never had to pay before.

But even if our nuclear arsenal could protect us from nuclear annhilation, what kind of life would be given us? Constant anxiety, cancers caused by radiation, birth defects, congenital cancers in our new-born children.

I'm honestly astonished that anybody values U.S. technological superiority in the arms race more than they balance human life and ecological balance. Think about what is trying to be saved with nuclear weapons, and you will see that in the process of making them, they are destroyed or cheapened.

What can be done in the context of this environmental impact statement? No. 1, assess the cancer rate among lab workers and Livermore residents. 2. Include in the report a statement that many medical experts question the actual safety of established safe doses of radiation. 3. The waste produced at the Lab do not cease having environmental affects once they leave the Livermore site. Assess the effects that these wastes have on the eco-sphere and state that no safe storage method exists for nuclear wastes nor can one be conceived of.

There was a recent inter-departmental federal report on the various modes of waste disposal, both proposed and used today, and all were found to be ineffective over the long term.

4. Assess the environmental impact of the arms race. 5. In your section on alternatives, support what you call the use of alternative technologies, and what I call peace conversion. Cite the Department of Labor study which showed that weapons development generates fewer job per dollar than almost any other form of government spending. The people of Livermore do need jobs, and with care and ingenuity, the 30 percent of the city's workers who work at the labs can be given work that is socially constructive and relatively non-polluting.

I was going to cite the up-coming mid-Peninsula Conversion Project Study, But Diane Thomas Glass did that better than I could.

6. One interim goal which I am sorry to say is compatible with the continuance of the arms race but would still make a great deal of sense to me and to the people of my group is the removal of all radioactive materials from Livermore and the Nevada test site. This is, of course, only a stop gap measure, because all the problems associated with waste disposal, et cetera, still remain. But removal would eliminate the spectacle of radioactive materials sitting on earthquake faults near a huge population center and it would stop the routine emissions of radioactive materials into the air and water of the Bay Area.

In conclusion, let me ask you to think open-mindedly about the vast human costs which the Labs, in their present form, exact from all of us, especially the workers and the people of Livermore. And please give serious consideration to the alternatives. Thank you.

MR. FARMAKIDES: Thank you.

Ms. Margaret Olney?

STATEMENT OF MARGARET OLNEY

MS. OLNEY: My name is Margaret Olney, and I live in Berkely, 2417 Blake Street, and I am speaking for the Berkeley Society of Friends and would like to speak very briefly on two topics.

The first is on the mission of the Labs, and I know this is not considered as part of the scope of the environmental

impact statement, and that you have limited power in influencing governmental policy, but it did seem that speaking at this hearing publicly would make known our questioning the wisdom of this policy, and you could transmit this information to the government which would help change -- which might bring about some change.

Also, I realized that there would be people here that work at the Labs and it might make them question what they are doing more seriously than they are now.

My feeling, and I think the feeling really of the Society of Friends, is that the violence, or premediated violence which is involved in doing research in the development of these very destructive weapons will promote more violence or counter violence, and that these weapons systems may for a time deter countries from attacking, and there may be for a time a balance of power, but eventually that balance will be broken and the attack will come.

I personally -- I think I'm a little bit more alarmed than some of the other people in the Berkeley Friends. I feel that our time is limited, and that if the work at the Labs continues very much longer, that there very likely will be an attack on Livermore which would involve the Bay Area and other areas around. And I feel that therefore, feeling this way, that this environment is in danger, and that I should speak out publicly on this. That is my first concern.

The alternative to this is to put energy and creative talent into disarmament and to figuring out ways of meeting violence in a creative, non-violent way which the human race so far hasn't done much of. But I think we have reached the point where we either do that or we exterminate ourselves.

It took me a long, long time to come to this view, and I don't -- I think that the motives of the people that work at the Labs and the desire to protect the country and so forth are admirable, but I do not feel that if they examined more closely and more intuitively the behavior of what -- actions of human beings and what courses of actions lead to what consequences, that they could continue to do the kind of work that they are doing. I think that outstanding people have come to this conclusion, such as Jesus and Ghandi and Tolstoy and others.

So the other concern is to do with underground testing which I realize is not on this Livermore site, but Livermore is responsible for underground testing. And I've learned that there is an alarming rate of radioactivity that gets into the atmosphere when these tests are done, and I'm not sure that the public is aware of this. And it seems to me to make known the amount of radiation that comes above ground when under ground testing is done would be a good thing.

And I also wonder about the effects of underground testing, if we are being naive in thinking that this is not

harmful. I wonder about the radioactivity getting into water that seeps into other areas, and how well can you keep isolated one contaminated underground area from another. I think maybe there have been so many geologists here, that maybe they can have the answers and maybe my fears are not important. But this is one of the concerns that I would like to raise.

MR. FARMAKIDES: Thank you very much.

The next person is Mr. Dan Hasley.

STATEMENT OF DAN HASLEY
PEOPLE FOR A NUCLEAR FREE FUTURE

MR. HASLEY: My name is Dan Hasley, and I live at Kresge College at University of California at Santa Cruz, and I represent people for a nuclear free future at Santa Cruz. Everything that I was going to say has been said, so I will just summarize it in two small points.

The first is that we feel that all radioactive substances at the Lab site should be removed from the site, and that they should be isolated from the biosphere.

And the second thing that we feel should be done is since the DEIS does not deal with the economic impact of the community on doing this, we feel that an alternative use committee should be set up in Livermore, and it should include members of the Livermore community and people who work at the Livermore Labs, as well as people in the Department of Energy.

Thank you.

MR. FARMAKIDES: Thank you.

Mr. Joe Ventresca?

STATEMENT OF JOEL VENTRESCA
HAIGHT ASHBURY NEIGHBORHOOD COUNCIL

MR. VENTRESCA: My name is Joel Ventresca. My address is 202 Grattan Street, San Francisco. I'm an elected representative of the 500 member, 19 year old Haight Ashbury Neighborhood Council, a San Francisco neighborhood orgnization. Members of our community group are concerned about nuclear energy and the research and development of nuclear technology. In fact, my neighborhood might have seen and acknowledged the writing on the wall when we voted 71 percent two and half years ago for the California Nuclear Initiative which would have placed serious limitations upon the development and threat of nuclear technology.

After review of the draft DEIS on the Livermore site, we have a number of concerns and questions. 1. Why isn't there a more complete record of cancer rate studies for the on-site and off-site populations included in the EIS? Comparison of on-site surrounding communities, regions, state and national cancer rates should be included. The Laboratory's rate of malignant melanoma is twice as high as that of the Bay Area and, perhaps, four times the national average. Melanoma, of course, is the most serious type of skin cancer. It is a fast moving, often fatal skin cancer. Malignant melanoma has been

9ncreasing around the nation by one to two percent a year. But in Alameda, Contra Costa, San Mateo counties, the rate has been going up five to ten percent a year. Bay Area rates for several different types of tumors are going up. For example, one cancer rates among women in five San Francisco Bay Area counties including San Francisco have tripled during the last 10 years. And between 1970 and 1975, the overall Bay Area cancer rate rose by about 10 percent, including double the melanoma rate.

With this information in mind, there should be a definitive statistical analysis of cancer rates at Lawrence Livermore Lab and comparing those cancer rates with surrounding communities, the Bay Area and the nation.

The second area of concern is why isn't there a fuller description of the 17 accidents at the Lawrence Livermore Lab listed in Appendix 3 C, page 3.3-1. Three of these accidents are explained away in one sentence. This is inadequate. Also, and very importantly, what was the criteria used to define an incident as an accident at the lab?

A third area of concern is this: 25 percent of 60,000 55-gallon barrels of nuclear wastes dumped 25 miles west of San Francisco, off of our coast, between 1946 and 1970 have broken open, releasing various types of radiation into the water and sediment around them. A state report found sesium 137, a dangerous radioactive contaminent in a fish sample off the California coast. Some of the barrels have

floated ashore. The concern is that the radioactive contaminents could work their way up the food chain to our dinner table. Were any of these nuclear wastes generated by work done directly or indirectly at Lawrence Livermore Lab? If so, how much or what percentage? If so, what do you, or what do they intend to do about this nuclear dump off the coast of San Francisco.

Earlier in the hearing, Mr. DuVal stated the dump site off the San Francisco coast did receive nuclear waste material from LLL, but it was a commercial site. What commercial corporation or a company is responsible for dumping Lawrence Livermore Labs' nuclear wastes off our coast? Does this mean since LLL didn't dump it there, it is not responsible for the waste even though the Lawrence Livermore Lab generated some of the waste that is there now?

It is a curious thing that of the 17 accidents listed in the appendix in the EIS, the draft EIS, this dump site is not listed. I submit if it was not an accident, then it was a very serious mistake which can have potential off-site impact, which is the definition used in the draft DEIS, the criteria for the listing of the 17 accidents.

Finally, I would like to introduce into the record a series of nine newspaper articles from the local press over the last two years concerning the high incidence of cancer rates on, near and around the Lawrence Livermore Lab. I hope

the information in these stories can be confirmed, revised or added to in your final EIS.

In conclusion, in regards to your staff statement, March 1979, page 29, concerning the 14 Lawrence Livermore Lab employees who have contracted melanoma, staff response states, "LLL has supplied the California Department of Health Services with employee records covering the period of approximately 10 years." I have three questions on this statement.

A. Which employee records were included for review by the California Department of Health Services? B. How many employee records are under review? And, finally, C., why aren't employee records covering a longer period of time than 10 years being reviewed?

MR. FARMAKIDES: Mr. DuVal, did you have a response sir with respect to some of the issues raised?

MR. DU VAL: With response to the last point, I would like to ask Mr. Olsen if he can elaborate on the aspect of the records that were submitted to the California Department of Health, or if he has other staff that he can ask.

MR. OLSEN: I can make the statement that we have the study going on with the State Public Health, and all of the records of all of our employees that we have are submitted to the State Public Health for working with them.

You have to recognize that a number of our employees who worked with us and then terminate, and have left the labor-

atory; we have no record of where they live, where they now reside, and that's the difficulty of going back to many years. But all the records of people who are presently employed have been furnished.

MR. VENTRESCO: Has there been any effort to search out former employees and where they reside now and try and find out their medical histories. You are telling me that you supplied the record of present employees --

MR. OLSEN: The thing you have to recognize is that the California Public Health Service has the Tumor Registry for the people that reside in this area, and so they have to work with those people. If we have an employee that works with us and chooses to move back to Washington, D.C. or some other area, it is very difficult to gain those records. We have to work with what is available, and we are working with the public health and we are giving them all of the records that we can possibly get or to gather for this study.

MR. VENGRESCO: Have you made any effort to get the medical records of former employees, or you have made no effort to do that?

MR. OLSEN: We have made all the effort to get all the records we can possibly get; that's what I am telling you.

MR. VENTRESCO: Thank you.

MR. FARMAKIDES: Mr. Kenneth Miller?

STATEMENT OF KENNETH MILLER

DR. MILLER: My name is Ken Miller. I'm an emergency room physician in Berkeley. I've been an emergency room physician for seven years. My major training is in internal medicine. I would like to address my testimony today to the disaster control plan section.

I would like to refer to a statement made in the beginning page of the disaster control plan which I think summarized an effect that Lawrence Livermore Laboratory's statements have on the public, which is to lull into a false sense of security; that there are medical facilities to handle almost any envisionable accident.

This is a very short statement. It says, "moreover, we have taken measures to minimize the consequences of those accidents that do occur." I endeavored to survey over the past two months the capability of handling an accident involving radiation contamination in Alameda County, specifically in Berkeley and in Livermore. And, in doing so, I surveyed many of the state and county and city agencies who are empowered with dealing with this problem. I also contacted every hospital in Alameda County and at least one emergency physician from each one. In addition, I contacted the fire departments, the ambulence, several police. And I tried to do this on sort of a grass roots level because I wanted to see just how effective and how capable the receiving facilities are. And I came up

with some very disturbing results, and I think these results should be addressed in the environmental impact statement, because I think that the very existence of Lawrence Livermore Laboratory and other such nuclear facilities poses a grave hazard both in terms of the long term effects and short term effects from accidents.

In my survey, I sort of used a transportation accident. I became quickly satisfied that on-site accidents are handled well at both the Labs. But my worry was really off-site accidents because those are the accidents that involve the people in the communities surrounding these Labs. And I did a little bit of research which took me to critical mass where they reported about 200 accidents had been reported to the Department of Transportation, some of which have resulted in contamination of the environment and of people involved in the accidents.

And I ran into the problem that they describe which was jurisdictional disputes. Throughout the chain of command at the state and county and city level, there is a massive array of jurisdictions. In the event of an accident, the Office of Emergency Services provided me with just a very brief listing of those agencies that would respond. I will just read a few of them. The Office of Emergency Services, the Fire Department, the Highway Patrol, the Sheriff's Bureau, the DOE, Health Care Services, Public Works, Caltrans, Flood Control, Explosive Ordinance Team, Agricultural Commission, the Bay Area Pollution

District, General Services and the Sanitary Districts.

The gist of that is that in the response to an accident I think it needs to be clarified who is going to be in control. It is not really clear. People at the state level said we can't lean on local agencies. And the law states that it is the local jurisdiction which is the supervening agency, so the city is responsible.

I contacted various agencies like the Fire Department and the Ambulence Services. The Fire Department has had some, at least, notification of what to do for radiation accidents. I talked to one fireman who has been with the Fire Department for 10 years. He has received one hour a year of a chalk talk without any particular drill. He doesn't know how to use a meter. And he says that the people he's talked to in the fire department don't either.

The ambulence service is a whole other thing. The only ambulence service that could possibly deal with an accident is the one from the laboratories. When I talked to the local ambulence companies in both Berkeley and Livermore, I got -- I hope it never happens and I wouldn't know what to do -- a statement of the management of contaminated injured personnel from Lawrence Livermore Labs says that the use of commercial ambulences will be held in reserve with the full knowledge that they have no training or facilities for handling problems involving radioactive contamination.

I think their recognition of this lack should have compelled them to assure that they did receive some training. And I think it is irresponsible in view of the fact that they are responsible for these accidents.

MR. FARMAKIDES: When you say "they," who do you mean?

DR. MILLER: I mean Lawrence Laboratories, the Hazards Control Division. I think some effort should have made to include the tri-cities ambulence in their training.

I got some very interesting comments when I was talking to the Office of Emergency Services at all levels. At the state level, one of them said, well, you know, Alameda County isn't exactly in the forefront of emergency preparedness.

At the local level, at the county level -- and I think this is a fair estimation -- the man said I feel like if we responded to an accident, we have sufficient knowledge to contain, evaluate and possibly decontaminate injured people at the site. But where do we go. He didn't know where to take them. And I agree with him.

I checked into every hospital, and I found no facility anywhere in Alameda County that is properly equipped to evaluate and manage a patient who is the victim of an accident, who happens to be both injured and contaminated, no proper facility.

The Commission of Accreditation of the Hospitals requires you to have a protocol. Every hospital did have a protocol. I must say that for them. Some of them will

decontaminate you and evaluate you in a parking lot, and I hope the weather is good. Others will do so in a morgue, which is fine if they contain the water. Some didn't care. Some didn't know that you should contain the water.

Facilities have been described, appropriate facilities, and it does require some money and some energy, but I think that this part and parcel of having nuclear facilities in your neighborhood. It calls for a two-room facility with one way traffic flow, with special filters in the air conditioning, or the capability of turning them off right away and the knowledge to do so, and containment for all fluids and waste. And it takes this to the personnel. Not one physician that I talked to had had any formal training in managing a person with radiation contamination. Furthermore, they knew of none of the courses offered. There are free courses offered by Oak Ridge and the state. The state's ones have been terminated this year, according to the state office, RADA, because of money.

The last thing regarding the hospitals and the medical personnel is that I sat in on disaster committee meetings, and I heard things like I am not afraid of anything I can't see. I asked surgeons what they do. Surgery is a real problem. If you have a penetration injury, massive bleeding, shock, you may need to go to surgery right away. There is not a surgical facility in this county, including the Naval Hospital, where my colleagues said, we'll let the Navy handle them.

There is no facility there for isolated surgery. So that if you were to have to perform surgery, you might contaminate your entire hospital. There's been no training, no drills.

I found it hard to address the problem of evacuation. Reading through disaster plans is in itself a disaster. It is very hard to interpret what is going on. Generally speaking, they outline the responsibility in the chain of command in the event of a disaster.

I read both the Livermore and the Berkeley disaster plans, and I couldn't find anv adequate elucidation of evacuation plans. Rumor has it that Livermore has an evacuation plan for a flood. I have their evacuation plan here. I don't see evacuation mentioned. Evacuation in the event of a large radiation cloud release or contamination, say, from a vehicle is an entirely different matter from an earthquake or a nuclear disaster or a flood. And I think that this should be addressed.

One of the comments that I got, and I feel this is really important. All along the chain is well, we don't have drills because we don't want to alarm the public unduly. And I think that there is a distinction between alarming and educating the public, and that is going to bring me to the last thing I have to say, the last area that I want to talk about.

And this is -- I would like to indict the Labs. I indict them because of omissions and because they lull us into a false sense of security. There is a complacency that filters

filters down from the experts. It filters down from the experts not only at Lawrence Livermore Laboratories, but also health physicists throughout the country who are invested in the nuclear industry.

I attended a conference in Utah last week on low level radiation, and this was very apparent. Of the 10 speakers who spoke, seven were gravely alarmed,and had studies which backed them up,over the dangers of low level radiation. The three people who minimized those hazards were all involved with the nuclear industry.

In the Times of 5 September 1978, I would like to quote to you an interview, a very short paragraph here. It is entitled "Nuke Emergency Game Plan Ready to Roll in Valley," And I quote: "If everything goes wrong at once,'Garberson said. I believe Jeff Garberson is the Public Relations Director. "Exposure would be well within the safety limit."

That is a pretty grandiose and broad statement. One person at the boundary of the labe, he said, would get one or two rems at the most. It goes on further to say that routine procedures for specific emergencies have been set up and evacuation contingency plans have been formulated.

I don't think that is completely true. There have been some contingency plans. I give it to Livermore's credit that they had the best in terms of a radiation spill for their disaster plan; it was two pages long.

I have some further information that I would like to impart to the people here, which I think further supports this indictment of the complacency of the experts and how it filters down and affects us all.

Yesterday, I attended an in-service education put on by a health physicist from Lawrence Livermore Lab. It was the first one conducted at Valley Memorial Hospital in Livermore, according to people who were there. I think it is no coincidence that these hearings are held today, and that was held yesterday. There was no physician in attendance there. There were six of us, mostly nursing personnel and janitorial personnel. I listened to the slide show and the presentation, and I found the same thing that I found when I talked to the radiation safety health officer at Lawrence Berkeley Lab, when I read the brochure handed to me from Lawrence Livermore Laboratory called "Living With Radiation," and when I read the guide to radiation protection from Lawrence Berkeley Lab, and that is that radiation isn't harmful. It is no more harmful than the sun.

I'm not sure whether they believed this, or whether it is a propaganda attempt, but I felt that there should be at least some mention of the vast array of medical literature which is accumulating, which indicates that radiation is in fact very dangerous; that it is not to be taken lightly. I believe that this flippant attitude descends throughout the

agencies. It descends into the health care personnel and the medical personnel and is directly responsible for the -- really the lack of preparedness we have in handling an emergency. I feel like the lab might be afraid to participate in wide-spread drills for fear that people might realize what it means to live next to a facility such as Lawrence Livermore Lab. I also feel that it is an insult to our intelligence not to provide us in a general public education way with materials so that we can decide on the basis of studies that are existent presently what is dangerous and what is not, and what needs to be done, rather than relying on -- I hope it doesn't happen. And don't worry. The DOE will take care of it. There are lots of experts.

I have some recommendations. That was my first recommendation. I really feel that Lawrence Livermore Laboratory and the health physicists at its disposal should take an active part in community health education, and that this should be unbiased; that they should inform themselves, which I feel they are not, and others of many studies showing the contrary to what they put out.

I feel like city and county agencies should be informed when there are big loads of radioactive material being transported through their borders. They are, after all, responsible in case an accident happens. They need to know.

It has already been addressed, the surveillance issue.

It is really atrocious. We have here the same situation that exists in the nuclear submarines, a captive audience exposed to a known danger, and they are not being followed up.

I would like to ask that Lawrence Livermore Laboratory address themselves to every worker with any known exposure who has ever worked there, with a really persistent effort at follow-up. It is very important to follow-up patients. We know this in medicine. If you don't know,and if you don't look, you have no basis upon which to say that your operations are safe.

I would like to specifically ask that the people who work at Site 300, the sub-data which is available to the laboratory be looked at and be studied as a sub-population with an inordinant and unique exposure to both radioactive substances and other hazardous material like beryllium.

I think that is all I have to say.

MR. FARMAKIDES: Ms. Grace Dilley?

After this presentation, we will recess for 10 minutes, then we'll come back and hear the comments of the limited participants who signed up during the course of the day.

STATEMENT OF GRACE DILLEY

MS. DILLEY: I have very few remarks because much of what I believe in has already been said. I wrote Mr. Pennington quite late. I will read briefly from this statement.

My feeling has been borne out by this hearing. My statement, as I said in my letter, will include the following.

My observation that the draft environmental impact statement seems to have been drawn up by the staff of the Lawrence Livermore Laboratory itself. We know now that it has been. I din't know definitely that it has been when I wrote this. This being the case, the result is not an impartial study and report.

The mathematical data therein can be and should be checked for accuracy. But the interpretation of these data would be highly subjective as would be any decisions for needed changes that might be indicated by them.

Second, I note in certain areas quite general rather than specific statements. For example, page 1-1, the fourth pragraph, quote: "Today, programs include magnetic fusion research, peaceful uses of nuclear explosives, bio-medical studies, laser fusion and laser isotope separation research."

Again, in paragraph 5, quote: "Anticipated national benefits include energy programs in geothermal, coal and solar energy." We need to know percentage-wise who much research in the process and plan in comparison with that for nuclear weapons research.

In this paragraph also it is clearly shown the non-objective thinking of those who wrote this report, quote: "Benefits to national defense have resulted from the nuclear weapons development program." This is an opinion, not a fact. However widely held, it is now increasingly challenged by a growing number of citizens, including many eminent scientists.

Third, and this has just been taken up by the last speaker, concerning health, and this is in the document after the comments came in and they were summarized. I will just read from one part of this. The bio-assay sampling and whole body counting programs are conducted to ensure the effectiveness of radioactive materials handling, and the containment procedures, and to assess routine or accidental uptakes, if any, that might have occurred. The vast majority, here, again, there are no specifics of how much that majority is, just vast. The bio-samples and whole body counts do not indicate any radioactivity above background levels.

During 1978, which was a typical year, there were no uptakes of radioactive material which exceeded permissible guidelines. Well, first of all, we don't know what permissible guidelines are. Dr. John Gofman says there is no permissible amount of radiation. But, secondly, in my own statement, this reference to background radiation and nothing above background radiation does not take into consideration the fact that background radiation in Livermore and the whole surrounding area now is much greater than it was 20 years ago because of the increase in atmospheric waste that is still floating around.

Background radiation goes up in the Bay Area with every single test in the Nevada test site. So that to say that people here are not getting any more than background radiation generally. If your "generally" refers to the Livermore valley

in particular, and also the whole Bay Area, you are not comparing what is credible, the background radiation for an area where there is no such facility. And even over the United States, background radiation is going up.

I think we know now from studies that have been done that, for example, in the state of Wahington, the mortality from cancer between 1972 and 1975 increased by 8.3 percent, whereas the general background mortality for those years, for the country as a whole rose only 3.4 percent. I think my statistics are correct. I'm quoting from memory. But it was over 8 percent in Washington. It was less than 4 percent. It was 3.3, I believe, for the rest of the country. So to say that the body counts here show nothing more than background radiation generally is really not a very accurate decision on the part of the people here.

I believe we must take much more into account here, at the laboratory. I hope that the suggestion of -- I've forgotten his name now. He was one of the first speakers discussing this, the seismology here, who said that there should not be any facility like this operating where the possible earthquake hazard is the greatest in the United States.

Why not, if this facility is to continue its experiments, it should not take the Nevada site, where the earthquake hazard is not so real and the population is so much less.

As has been pointed out here earlier, one serious

earthquake happening can leak air into the building where the plutonium is, and we can have a major disaster for a very largely poulated area.

I do not think it is right for the labs to say, or the Lawrence Livermore staff to say that it probably won't happen. It is all right for a man to say it probably won't happen to me. It is not right for a man to say in case it happens, 5 million people may be exposed to lung cancer. No one may take that chance for that population. No one may take that chance for any other human being except him or herself.

And this is a public agency which is supported by public funds. I think this is the message that must be made to the Nuclear Regulatory Agency, to the Department of Energy, and which must be demanded of this facility.

Thank you for letting me speak.

MR. FARMAKIDES: We will now recess for 10 minutes. We'll reconvene and we will take the limited participants in order.

(A brief recess.)

MR. FARMAKIDES: Mr. Page, could you proceed, sir?

STATEMENT BY DAVID PAGE
MOUNTAIN PEOPLE FOR NON-NUCLEAR LIFE

MR. PAGE: Yes. My name is David Page, and I'm here representing Mountain People for Non-Nuclear Life. Our group's address is 260 Desar Way in Felton, California.

I'm a Vietnam era veteran who has spent my military years at different hospitals in the medical corps, and since that time I've trained as an emergency medical technician and I've worked in that capacity. Because of this background I would like to offer my perspective concerning the disaster control plan.

The introduction to the disaster control plan states that "It cannot prevent accidents entirely and occasionally mechanical failures and human error do result in accidents." And then it goes on to say, "This organization must be able to cope with the small and moderate accidents that most frequently demand its services, as well as the less frequent but more severe accidents that can possibly occur."

While the authors of this plan consider it adequate, I'm afraid there are problems with it, many of which perhaps cannot be resolved by any emergency procedures plan.

I would like you to imagine as well as you can the situation that we're trying to describe. This is a difficult thing for most people to imagine, and this is the point I would

like to stress, because such events are out of the ordinary, and only a few people have the ability to be calm, organized and active in these situations because so few of us have experienced disasters. This is an important point because the samepeople that would be involved in a disaster are these same ordinary citizens.

Immediately after an accident, whatever the cause, the fire department is to be contacted, and after their arrival on the scene they are to determine if a disaster situation exists. The emergency dispatcher can then contact the primary disaster control group by radio.

At this point I would like to discuss a hyopthetical combination of a natural and man-made disaster -- say for an example, an earthquake that ruptures Building 332.

Since the disaster plans would be geared for a maximum possible, not minimum possible type of catastrophe, I feel that referring to such an earthquake is certainly appropriate. Now, the primary disaster control group to be contacted consists mainly of the people who are listed on the top line of page 3(b) 9 in the DEIS. These people can be contacted by radio at any time, according to the plan.

Does anyone know if this is correct, that the people listed are within 24 hour radio contact of the emergency dispatcher?

MR. FARMAKIDES: I don't know. Mr. Du Val, or Mr.

Olsen, does anyone --

MR. OLSEN: Yes. I can answer that. All of the people who were on the disaster organization have a disaster page. It's a system on top of Mount Diably which will catch these people in this whole Bay Area. And all of the people are doubled up, so that if we're unable to get one individual because he's out of town or something, another individual will respond.

MR. PAGE: Right. However, according to the DEIS, two important groups are not listed in the 24-hour radio contact. One is the support groups who are listed here as "riggers, welders, plumbers, electricians, carpenters, laborers, truck drivers, and others." And also, the technical staff who are experts in the fields of physics, chemistry and engineering and serve as an advisory body to the disaster control director and advise him of the possible effects or conditions that can arise during a disaster.

Also in the DCP general operations plan it says that -- "All laboratory scientific and technical departments are required to assist as requested by the individual coordinating the actions of the disaster control team at the scene of the accident." However, the majority of all the laboratory scientific and technical personnel, I assume, are only present at the lab for 40 hours during each 168-hour week.

I would like to say, in case it's not obvious, that

the reason for using radio instead of telephones is that in our assumed instance the telephone lines may not be standing.

It is possible in an emergency situation where there is advance warning, such as at Three Mile Island, that these disaster workers could be easily contacted. But in this hypothetical case I'm talking about, it's a different story.

Contingency plans in case the director's technical advisory body is unable to be contacted -- it may be that such plans are cost prohibitive. However, it would present in a possible disaster a serious problem.

Another point I'd like to bring up concerns fallout shelters. It states also here in the B Appendix that "The laboratory maintains a number of fallout shelters capable of housing 14,000." This is a projected maximum number of people that could survive for two weeks in fallout shelters.

However, there are 48,000 people living in the city of Livermore, and obviously radiation that would endanger the city enough to live in fallout shelters would also contaminate areas outside the city. And there are no plans for maintaining fallout shelters outside the city.

The DCP goes on that "Each shelter manager, however, is authorized to direct personnel to other shelters when his shlter reaches its maximum capacity. Now, if you can imagine that scene: I propose that many of the fallout shelters would not be useful because there would be many people fighting

each other to get in the door. And in that case the doors simply might not get closed in time to shield the people from the radiation. Also, the people that do get in might end up worse off because "Shelter areas include basements and other shielded areas of buildings. Therefore, some protection against a blast is provided." However, I don't believe that many of these basements have air filtration systems such that the inhabitants don't breathe radioactive air. And what will they do after the two weeks of supplys are up?

Also, the report states, "The police department is responsible for developing plans for traffic control during air raids when employees evacuate buildings to take refuge in the shelters." This brings up another important point. In a disaster like we're talking about, major freeways could be damaged by the earthquake. Even if they were all undamaged, they would probably be perhaps useless, if not totally useless, because of several other factors; The earthquake could cause much confusion among the populace. When this distraught, shocked populace learned of the evacuation notice because of the radiation, they would not be likely to obey traffic laws. And in this case, it would only take one or two accidents on any freeway to completely clog that traffic artery. And there aren't that many traffic arteries that bleed out of this area, so we're talking about an entire city closing in on itself.

The incredible chaos that would exist is difficult to comprehend. But if you will ponder it for a moment, you will probably see that an evacuation quick enough for people to escape contamination is highly improbable. So what then? Well, we can't hear, see or smell or know where radiation is without a special instrument. Beta and alpha radiation are most damaging when we ingest or inhale it. Gamma radiation penetrates our tissues, but it is not as dangerous as the alpha or beta radiation.

If people were told of the danger but they couldn't get away from, say plutonium dioxide in the air, they would be exposed to a high level of radiation. To quote from Dr. Helen Caldicott, a pediatrician at the Boston Children's Hospital Medical Center, "Such exposure to radiation kills all actively dividing cells in thebody. Hair falls out. Skin is sloughed in big ulcers. Vomiting and diarrhea occur. And then as the white blood cells and platelets die, victims expire of infection and/or massive hemmorage."

They would die within two weeks, not immediately -- some. An extremely high dose of radiation causes confusion, stupor, psychosis, fever and death within two days -- not two weeks. Less radiation causes leukemia and cancer.

Of course, the land would also be affected and considered useless for many years. If the people in any airtight shelters survived the two weeks and were able to come out at

that time, the psychological trauma could be worse than the physical. What I'm talking about is loss of friends, loss of homes and loss of areas that you're used to -- a total chaotic situation.

To conclude, as far as I can tell, all of these important deficiencies have not been yet addressed. Perhaps they cannot be adequately resolved. Or at the very least, I would sugges that the DEIS must come to terms with these questions and let the public know of these problems if they are indeed unresolvable.

MR. FARMAKIDES: Thank you, sir. Did you have any comments? Thank you again.

We're going to now start the limited participants who signed this roster. The names are Calvin Wolfe, Mary Sparks, Angie Patterson, and Paul Tule. Mr. Wolfe? Mary Sparks? Angie Patterson? Mr. Paul Tule.

STATEMENT OF PAUL TULL

MR.TULL: Paul Tull, 2243 Minden Street, Livermore. Well, I'm still not ready, but in most cases I've been able to speak better extemporaneously than if I wrote it down. Now, we have been speaking of radiation as though it were a tangible substance. But radiation, even from the biggest fusion furnace we have available to us is generally considered to be sunlight. It enters over into the ultraviolet radiation

and it occurs in the infrared. Now, the alpha, beta and gamma radiation from nuclear materials also is a vibration, an intangible substance that, for want of a better name, we've called ether. It cannot be ingested. The only way it can penetrate is to actually go through the surface of the skin.

However, the source of this radiation is something that we can ingest by breathing, by eating, by drinking the water in which it may have been contained. Now, that has bothered me considerably. I worked up at the Lab up until 1960. I took a medical leave. Well, I actually wasn't allowed a medical leave. I had to quit in September of 1960. I noticed the first accident they have in the book here occurred in November of 1960. I don't think I had anything to do with that accident. I was away from there then.

However, in looking back over it, I did get caught in an accident. A good friend of mine working in the machine shop, after he had quit out there and was tying of cancer, told me about machining a piece of the material that was code named "lion." I found out later that that was the code name for plutonium. The material coming off the tool bit -- he was turning it off the lathe -- the material off the tool bit caught fire and burned, just like a piece of magnesium.

Now, I was in that building. And about two weeks later I was called back to be examined for beryllium poisoning. I wasn't wearing a badge at the time. And as a matter of fact,

it was only three times that I was required to wear a film badge, and they never showed. I later found out that those film badges weren't a very accurate measurement of radiation in any case.

So, for various other reasons, including trouble with my back and shoulders, I quit in September of '60, one of the reasons being that things that were happening there that the public needed to know were covered up by a cloak of secrecy, secrecy that was only to keep the public off the neck of the officials in the lab.

Now, I got ahold of this EIS two days ago. I still haven't gotten through it. But I find that it can be categorized under something that a member of the Nuclear Regulatory Commission stated in the "Valley Times" for Wednesday, April the 11th. He says that he had worked for 22 years -- and it looks like he came from the Atomic Energy Commission over to the Nuclear Regulatory Commission. And I have found that many of those people are from the Atomic Energy Commission that was disbanded because it had lost its credibility. However many of the people came right on through. They didn't even change their job. And this man didn't either. His name was Englekin (?).

The thing that he said there was, "Fire is a very dangerous thing, but people learn to control it and live with it." He further stated, and this is a direct quote: "Radiation is no different."

I agree with him. I started out by saying that radiation is an intangible vibration in what we call ether. However, that does produce ashes, and those ashes can last for a quarter of a million years before it reaches half of the radiation that it had when it first was created. And those ashes can be ingested and are the source for the long development time required to produce cancer.

I find that radon, which is universal in the world here, degenerates in two-and-a-half days of its half life. In two-and-a-half days it produces particulate matter that can also be ingested.

Well, I have a direct quote from Dr. Edward Teller here, too, the Father of the H-Bomb, concerning the Three Mile Island Nuclear Power Plant. Teller said that evacuation efforts could have been more dangerous than the accident itself. There was no damage at the power plant, except to the pocketbook.

Now, that's fine. However when it first came out, they said "meltdown." They they denied a meltdown. They said iodine was found in the milk of the cows, radioactive iodine. Then they said no iodine. Then they said radon was released. And then they denied that, and they said that that cloud that went over Maine was from the boilout of the rocks that occurred every year. However, such a boilout never occurred prior to this time. And radon, as I said before, is a material that

degenerates into particulate matter. It's a gas to start with, and we can breathe it in during the time it deteriorates or changes to another material. It can lodge in the lungs, which are awfully close to the bloodstream. The bloodstream itself could carry a bit of that radon down to the reproductive organs and mess up the DNA molecules, and we can have an increase in mutation.

The increase in mutation, I maintain right now, is sufficient that it should be measurable -- a measurable quantity.

There were a couple of other things that I had in mind, but what I'm saying here and now is that this DEIS is written, first to confuse, insofar as it is not written in measurements that are prevalent in this country today. Now, to another scientist, that isn't too bad. To a guy that remembers a bit about his metric system, over and above the fact that you can count it up on your ten figures, can figure out that 1.6 kilometers happens to be a mile, and 16 kilometers is 10 miles. And 50 kilometers -- no, 80 kilometers is 50 miles. That's the first thing. And then every other measurement in the DEIS here, if you're an old codger like I am who never really did convert to metric, has to convert.

Then people in authority, without ever signing their name, say the same sort of statement that you get from other people in authority, such as Mr. Englekin, Mr. Teller, and

the various people back there on Three Mile Island. Incidentally, at Three Mile Island they said they'd taken into consideration all contingencies. They could handle the thing. Well, fortunately they did. But they didn't handle the human factor, which was the thing that actually allowed the meltdown.

So what I'm saying is this: We have to question the reliability of those in authority, particularly those whose job, whose salary and maybe livelihood depend on it, who have invested so much time and effort in learning how to operate with this stuff. And I mean they have not learned to operate totally with it.

Instead of making the statement that "The radiation is such-and-such at the boundary of the lab," they do not mention one bit of theparticulate matter that came out in the bodies of quite a few of the people that worked at that lab.

There was this fellow that had this fire on the lion on his lathe. We had another one two years who died of cancer. Both of them drank themselves to death. So on the death certificate, neither one of them died of cancer, but they had it. Both of them were good friends of mine. But during the process of dying, they drank themselves to death.

In 1959, I believe, the -- cyclotron. We still had the cyclotron there. We also had a linear accelerator. There were two Mexican ancestry sweepers. They had been raised in Mexico, though they were born American. They could not read

the language. They could not read the radiation signs. They heard what their boss had told them about. They were sent into the target room of the linear accelerator that night to clean it up. It was already set up with a target. The physicist was fooling around after hours and he looked at the TV readout. He didn't see anybody. He hit the alarms and fired the accelerator. Those two people were irradiated. They died in about six weeks' time, six weeks later. Their parents were paid off, and they were already back in Mexico. Now, that was before 1960.

There was a case wherein thorium, I believe it was, was found in the water table downstream from the lab, exterior to the lab's boundary. Those are two cases that I know happened before 1960. Of course, they're not in the book, because they cut it off about that time. I'm questioning the credibility of those that are policing themselves.

MR. FARMAKIDES: Can we ask the staff to respond to that, sir?

MR. TULL: If you will.

MR. FARMAKIDES: Mr. DuVall, did you hear the comment made by Mr. Tull? He's challenging the statement made earlier that there were no off-site -- I guess no off-site impacts before 1960. And he presents two cases within his personal knowledge that he says are in fact off-site impacts. Is that correct?

MR. TULL: That's right.

MR. FARMAKIDES: Do you know either of those, those two incidents?

MR. DUVAL: I'm unfamiliar with either of those incidents, Mr. Chairman. Are you including the accelerator accident that was described as an off-site accident?

MR. TULL: Those men went off site and were buried off site.

MR. DU VAL: Well, I didn't mean that in a bookkeeping sense, but only in the sense that the tabulation in this book is not intended to be a compilation of all accidents, accidents with oxide implications.

MR. TULL: Actually, Mr. Farmakides did not include the other two that I mentioned, both of them good friends of mine, the machinist and Nick Kristofilis.

MR. FARMAKIDES: Well, these are the people, now, that you mentioned were on site. That's the distinction with respect to that listing that you made reference to earlier. I think Mr. Du Vall's point is that that listing was of off-site impacts. Isn't that correct, Mr. Du Val?

MR. DUVAL: Yes, sir.

MR. TULL: If you get to off-site impacts, we have to get to the thing that Mr. Schwartz was trying to bring forth along about noon, wherein we do get off-site impacts from the very development that is done at the Lab. As a matter of

fact, we have gotten so much off-site impact that we are now using very blithely -- they use the amount of plutonium background, and before we started playing around with this stuff, there was no plutonium in our ecology -- that is, that could be measured. Plutonium has been the result of the explosions and the use of atomic reactors. And that has become a means of measurement.

MR. FARMAKIDES: All right, sir. Did you have anything else?

MR. TULL: Well, I do. But I don't remember what it is now. I'll think about it tonight and stay awake.

MR. FARMAKIDES: All right, sir. Mr. Watson? Mr. Jim Watson? Mr. Vernon Brechin? Ms. Celia Baker?

STATEMENT OF CELIA BAKER

MS. BAKER: My name is Celia Baker, and I live at 541 Bell Avenue in Livermore, and I wanted to say something about not many people from Livermore being here. And I think partly it's because they don't know about this hearing, and I think it's partly because they don't want to know about what goes on at the Lab. And I wrote something down.

My husband has been employed at the Lab for over 12 years, and it makes it a little difficult for me to be here. But I wanted to say two things: Firstly, Livermore suffers from one of the worst smog problems in California. Yet every

day hundres of single passenger cars travel to and from the Lab. And I just got the report today. I just saw the report. And I didn't see anything in there at all about the fact that Livermore has a bad smog problem. That's a question I wanted to --

MR. FARMAKIDES: Would you continue, and then we'll --

MS. BAKER: Yes. I think that the Lab should provide a convenient mass transit system. I think that might help. You have no comment?

MR. FARMAKIDES: Well, we'll see if we want to ask them for comments. You're not really a full participant. You're at the end of the session. And if we feel that there is some substance to be brought into the record with respect to the DEIS, we'll ask the question. But why don't you go ahead and finish your statement, and we'll talk about your questions.

MS. BAKER: The cars are mentioned in the report, but not --

MR. FARMAKIDES: Yes. Yes.

MS. BAKER: And the last thing, I put that I believe that almost all the dangers mentioned today would be alleviated if the Lab was genuinely dedicated to solving life problems instead of creating instruments of death.

MR. FARMAKIDES: Now, let's get back now to the point that you raised. You're suggesting, as I understand you, that the use of vehicles at Livermore Lab is something that

should be discussed in the Draft Environmental Impact Statement as a smog producer?

MS. BALL: Yes.

MR. FARMAKIDES: Well, I think it was discussed. But you're saying it's inadequate?

MS. BALL: I didn't see anything in there about the smog. I mean, Livermore is supposed to have the third-worst smog in California. Well, the people going to the Lab -- I've watched them. There's one-passenger cars, hundreds of them, going to and from every day. And I think that that contributes to the problem in the Valley.

MR. FARMAKIDES: Do you have any comments on that, Mr. Olsen?

MR. OLSEN: Let make some statements. Livermore is aware of the smog problem. We're aware of the problem that you state, that many people drive to work singly. Now, the Laboratory is doing a number of things to try to encourage car pooling. We have set up car pools to assist people in getting car pools. We've set up preferential parking for car pools. We have worked with BART and the city in trying to encourage buses. If you'll notice, we have the special buses that go to Livermore during the lunch hour so that people who want to go down and do business can do this, so that therefore they can ride a bus to work and it gives them an opportunity to go to Livermore and take care of business.

We're setting up what we call van pools. And we are helping the people finance and set these things up. And we're doing everything that we can to encourage the people to go into buses and vans. So we're working with the city, with the county, with the BART system and with our employees and with the DOE to do everything we can to try to relieve this problem.

MR. FARMAKIDES: Did you have any other questions, Ms. Baker?

MS. BAKER: No, thank you.

MR. FARMAKIDES: Thank you. James Smith?

STATEMENT OF JAMES D. SMITH

MR. SMITH: Okay. I've been here all day, and I've been listening to the opinions of professors, environmentalists and physicists.

MR. FARMAKIDES: Would you please give your full name and your address?

MR. SMITH: Oh, sure. My name is James Douglas Smith. I live in Livermore, California. My address is 594 Escondido Circle.

I've listened to all the testimony today by experts and environmentalists and people that are just concerned, and one of the questions that I've come up with that I'd like to ask you is, what are the probabilities of an accident happening? Of an earthquake -- well, which -- Well, it's been

discussed about the earthquake. The probabilities of an earthquake cannot even be really calculated, not even with, you know -- I'm sure a lot of people in San Francisco would like to know the probabilities of an earthquake -- of the next one, I guess.

But what I'm stating is human error, or some kind of an event that would -- that the release of radioactive material into the atmosphere -- what are the probabilities of that happening?

MR. FARMAKIDES: Mr. Smith, could we also follow the same procedure? Could you go through your entire presentation and then we'll deal with the questions?

MR. SMITH: Okay. That's fine with me. There was a suggestion earlier about moving radioactive testing out of the Livermore area. There's a lot of people here that don't seem to realize that Livermore is a Lab boom town. What I mean is, back in 1950, I guess, is when the Lab came in. But when the Lab came in, this was just mostly an agricultural type community, like Brentwood up north.

When the Lab came in, this town's business sprang up overnight. Now, what I'd like to know is -- well, I'll just make my statement. I'll ask my question later. If the Lab moved out of Livermore, a large part of the population of Livermore would move out along with it. This would have a devastating effect on the town's businesses and services,

which means unemployment of a lot of people who provide the businesses and the services to the community of Livermore, which is mostly composed of Lab personnel.

Another thing, too, that I'm bringing up is that I am reassured that I'm safe. First of all, my parents work at the Labs, both Sandia and Lawrence. And I'm reassured that I am safe, mainly because a majority of Lab personnel live in Livermore. Now, if I think that if many people in Livermore, of Lab personnel, thought they were in danger, it would be idiotic for them to live in Livermore. I know there are going to be hundreds of people disagreeing with me on this, but that's my feeling.

I also feel that if there was a danger present, that the Lab personnel and the people at the Lab would make rapid movement to correct the problem. See, the thing is, there's people here that are from outside areas that do not live in the Livermore area, coming in and saying about the problems of the earthquake fault and of the possible release of radiation.

Well, it's all right for them to come into the valley and state the problem, state their point of view. And they have the right to do it. But as a Livermore resident, I feel that, you know, I am more concerned with -- well, okay. I'm really nervous, because I'm not used to speaking in front of people.

MR. FARMAKIDES: That's all right, sir.

MR. SMITH: Okay. I really feel that the Lab people

would not put themselves in jeopardy. It just doesn't make sense. It's idiotic. If there is a problem, it should be corrected and I'm sure that the Lab, if they do find a problem, through, hopefully, future research into the problem, that it will be corrected.

As a citizen of Livermore, I am concerned about the safety of Livermore, but I'm also concerned about the economic effects of the Livermore Valley, which is not only Livermore, but also includes Pleasanton and Dublin and San Ramon. A lot of the Lab people also do business in the other towns, too.

I feel that if you are going to do an in-depth study you had better have a team of scientists that are not appointed by the Department of Energy, but then again not appointed by environmentalist groups and anti-nuclear advocates. I believe that the research team should be composed of personnel from geologists to ecologists and I think you should also have an economist on that board to evaluate what would happen if you closed down the lab like you did with -- I'm not saying "you," but like they did, whoever did, with Vallecitas. Vallecitas has been closed for a year due to the fault testing. And I know a person personally that worked for that company. He's been out of work for a year, and it's having a grave impact on his pursuit of happiness, shall we say.

I think we should have an economist on the panel to decide, what would be the effects of Livermore's business

aspect? What would happen to the town of Livermore? Would it turn into a ghost town like the silver mines and the gold mines of 100 years ago? I hope not.

I've lived in this town for eleven years, and I might live in it for another eleven. I like this valley. I'm directly related to -- if the Lab closes, I go. That's all there is to it. And I don't want to leave.

Now, one thing that's got me a little mad, and that's probably why I'm a little nervous -- is that all of a sudden environmental groups that don't live in the Livermore area have taken a great concern suddenly about our environment. For the past six years we've had the third worst smog problem in California. L.A., I hear, has the worst. and San Jose has the second.

But I haven't heard one cry from any of the environmentalist groups about our smog problem. Now, maybe the have, but boy, they haven't made as much of a noise as they have here. You know, frankly I think that it's all right for them to come in and talk. But I think that mostly what spurred them on is the three-mile incident, which has spurred on a lot of anti-nuclear opinions lately. And I don't think, if it wasn't for that that we would be having as much anti-nuke people in here tonight.

The one thing I would like to ask -- and it was a question I was going to ask Dr. Schwartz, but he doesn't seem

to be here now. I guess he's home eating, which I wish I was doing. It was from his earlier statement, which caught my interest. And he was saying that we might have -- in effect of world affairs. I know that that doesn't pertain to what's happening here directly. But he said that in 30 years that there would be nuclear holocaust, I guess, in his predictions.

The one question I'd like to know is where is he going to be in 30 years, you know, when the bomb does drop. But then again, he isn't here to answer. That's really irrelevant.

The main questions I have are: What are the probabilities of an accident happening out at the lab?

MR. FARMAKIDES: When you say "accident," that's the second time you've raised that same question. And we'll ask that for purposes of the record, that perhaps there be an answer. But first of all, what do you mean by "accident"? What are you talking about?

MR. SMITH: Okay. What I mean by "accident" -- I know it's a wide range. What I mean by accident is, for one, I know we can't state, like a missile or a bomb or some kind of exploded device by a foreign country falling on the Lab. I don't want to know that. I want to know human error, I want to know the possible change of a system failing that would release radiation into the atmosphere. That's what I want to know.

MR. FARMAKIDES: Mr. Du Val or Mr. Olsen?

MR. DU VAL: Let me start with a point, and, Mr. Olsen, please to add in when I am concluded, if there is something that you would like to say.

With regard to the art and science of risk assessment, I wish we were advanced to the point, from my knowledge of it, where either from the earthquake point of view or the maximum kind of accidents we were talking about, we were in a position to talk statistically about probability of occurrence. To my knowledge we are not. The maximum credible accidents that were postulated in the DEIS take the worst credible case of a number of different situations and run them through scenarios and identify consequence. But to get to the total hazard, the total risk, you have to multiply consequence by probability. I don't think there is any probability number that is associated with that, because, really, the only way to get probability statistics is to have a history of events of that you can look back on and say, this happens this frequently and that frequently and use that as a basis. Otherwise, you are into theoretical probability assessment.

MR. SMITH: Okay. So you are stating that there hasn't been enough accidents to calculate a probility of one happening? Is that what you are saying in one way?

MR. DU VAL: That is a paraphrase of what I said that is pretty close. I say that without saying that I wish there have been enough. I'm saying in the absence of accidents

statistics, all we can come up with are theoretical probabilities. But we don't count on it. That's the point. The point is that we identify the maximum credible accident and take measures so that the consequences do not affect people. You minimize those effects, so if it happens we can deal with it. Therefore, our approach is not based on a warm feeling that the probability is low. We assume that it is going to happen and look at the consequence and take steps to contain those consequences.

MR. OLSEN: Let me answer it in a way that I'm -- I've sat here all day, and I've listened to all these things like you have. Let me try to answer you in a different way .

Let me take the plutonium building. as an example, to try to explain this. You heard today from the various people here who postulated a catastrophe. Now we, in a laboratory, as Mr. DuVal has said and DOE, have tried to write into our documents maximum credible accidents can occur. Now in the plutonium building, we have, as they stated in our Hazards Analysis, the the work status we can have as much as 40 grams of material. We normally work with this is -- or 40 kilograms. We work with this in 10 dilogram quantities, in work stations.

Now this material is worked in the side of a glove box, in an atmosphere of not air, and we do this because it keeps the plutonium cleaner. Now we work with the plutonium in a metal, and the statement has been made here that metal in plutonium will burst into flame. Now it is true that if you

have chips of metal, it will burst into flame. But if you have a solid piece of plutonium, it will not burst into flame.

Now in the plutonium building a statement has been made, if a crack occurs in the building, we've got a catastrophe. Well, that is not true, because the building is a second container. You have the plutonium inside of the glove box, and if a crack occurs in the glove box, the glove box is at a lower pressure than the room, and the air will rush into the room. If a crack occurs in the building, the air will leak into the room not out of the room. And so these things are as controlled as we can.

Now we've tried to take a maximum credible accident, and we did this by going to our heavy elements building, and we took the amount of material that we worked there, and we made the assumption that we lost the building, that that material burned, and you get so much into the atmosphere. And this is the maximal credible accident that we predicted.

Now we really see no credible way to get a type of accident that was proposed by these people. Now they don't believe us, and they don't believe our credibility . But these are the facts, the statements, the best that we can calculate with the best experts that we have. And these are the things we are trying to bring forth. So I would recommend that you read the maximum credible accidents and try to understand those.

Now, as you also have heard, there are a lot of things that we cannot prove. You've heard the seismic people make all kinds of predictions. And that's the work that we have to work within. Now, we have made studies with people to try to find if there are faults under our sites. We've had the Bloom Associates make those. Now, they have not found any indication of them there. As anyone tells you, the harder you look, you may find them. So no one can stand here and prove to you with absolute probability that nothing will ever occur.

But we've taken all the precautions that we can to do this. Now, we've had accidents as time has gone on. And every time we have an accident, we try to find out what happened and correct it. Where we have had human errors we try to correct it such that a human error cannot occur, one single error occur and cause an accident. We try to make it multiple human error, so that there is always protection. So the chance of a catastrophe happening is minor. But we do have a number of accidents. But we try to keep them inor.

MR. SMITH: Okay. I have some questions that you brought up, some questions resulting from what you said. You said if you stored in --

MR. FARMAKIDES: Would you address your questions to us, please sir?

MR. SMITH: Oh, sure. Okay. I have some questions

on what Mr. Olsen had brought up. Do they store it in solids most of the time?

MR. FARMAKIDES: Store what?

MR. SMITH: The plutonium.

MR. FARMAKIDES: Mr. Olsen?

MR. OLSEN: All the work we do with plutonium is in the solid metal. We work with very little liquid systems. Essentially it's all metal.

MR. SMITH: Okay. Oh, one other question. I was wondering: Do any of the gentlemen on the Board over there live in Livermore?

MR. FARMAKIDES: You mean any of the people on the staff panel?

MR. SMITH: Yes.

MR. FARMAKIDES: I don't know. Mr. Murphy, Mr. Olsen, Mr. Du Val, do any of you live in Livermore?

MR. DU VAL: I do not live in the city of Livermore. I live in the city of Clayton, right on the other side of Mount Diablo.

MR. SMITH: I see. Did any of you live in Livermore at one time?

MR. OLSEN: Yes. I lived in Livermore when I first worked for the Laboratory from 1954 until 1957, at which time I went to the Nevada Test Site and resided there in the testing program until 1962. Then I moved back and I chose at

that time to live in Alamo. And I chose to live in Alamo because in Livermore I couldn't find a half acre of ground to settle on.

MR. FARMAKIDES: Mr. Murphy?

MR. MURPHY: I do live in Livermore.

MR. FARMAKIDES: You do live in Livermore. Mr. Smith, we'll have another question, sir, and then --

MR. SMITH: Mr. Murphy, if you live in Livermore now, do you feel safe living in Livermore right now?

MR. MURPHY: Yes.

MR. SMITH: Okay. That's all I have.

MR. FARMAKIDES: All right, Mr. Smith. Thank you very much, sir. Mr. Dougherty, Mr. Hughes, Mr. Dickenson, and Mr. Eller? Mr. Dourgherty? Mr. Hughes? Mr. Dickinson? All right. It will be Mr. Dickinson and then Mr. Eller.

Could you please give me your full name and address, sir?

STATEMENT OF JOHN BRUCE DICKENSON

MR. DICKENSON: Good evening. My name is John Bruce Dickenson. I'm a resident of Berkeley, California on 2509 Derby Street. And I'm here as a member of the Conservation of Natural Resources student organization from U.C. Berkeley.

MR. FARMAKIDES: How much time do you need, sir?

MR. DICKENSON: Roughly five minutes.

MR. FARMAKIDES: All right, sir.

MR. DICKENSON: One question. Is this being recorded for the record?

MR. FARMAKIDES: Yes, it is. You're being recorded.

MR. DICKENSON: Okay. As students at U.C. Berkeley and as members of the Bay Area community we are concerned with the extreme unsafety of the Lawrence Livermore Lab and its potential effect on our lives.

After careful examination we conclude that certain aspects of the DEIS evaluation of LLL are inadequate, inappropriate or deficient for the following reasons:

(1) Environmental impacts of routine and accidental emissions of radioactive and toxic substances into the ground, air and water were not adequately reported in the DEIS.

(2) Inadequate standards employed for determining the maximum credible accident for plutonium sampling procedures and for monitoring levels of exposure to radionuclides. For example, the maximum credible accident for tritium is far too low, in light of two accidents involving tritium releases that occurred at the Lab on August 6, 1970 and in January, 1965 with releases of 300,000 curies and 350,000 curies respectively. These amounts are on the same order of magnitude as the lab's theoretical worst case for tritium releases.

As for plutonium sampling procedures, why isn't respirable dust sampling used, instead of whole soil sampling? The effective levels of plutonium have been shown to be as

much as 285 times greater than those obtained by whole soil sampling. This was done by Mr. Carl Johnson, who I think spoke here earlier.

(3) Seismic safety at the Lab is shown to be very low, or indeed questionable, bu independent analysts, but not by the Lab's DEIS evaluation. We support the views of Friends of the Earth's critique of the DEIS and we would urge you to fully consider it. We also recommend further seismic studies by an independent group or agency, such as the USGS.

(4) Regarding insufficient health and safety standards for Lab personnel, we recommend reexamination of your standards and that you use the new standards on exposure to low level radiation and beryllium.

(5) Regarding insufficient analysis of transportation and storage problems and safety. There is no approved permanent site for plutonium and other transuranic wastes at the Lab. What happens with these wastes and who gives authority for removal and transport of them? Once the transportation container tests are completed, does the Lab plan on resuming air shipment of nuclear material? Has there been notification sent to all counties through or over which the Lab ships radionuclides? Do each of these counties have an emergency plan to deal with an accident involving the breach of these containers? These questions should be more thoroughly dealt with in the final EIS.

(6) Regarding inadequate preparation of emergency response plan: What plan is there for local residents within a ten-mile radius? The only plans are those prepared by the local communities, such as Livermore, and they are totally inadequate, as we have heard from medical people just earlier.

(7) Regarding incomplete information on incidents of cancers in the Livermore Valley and at the Lab, we recommend that a full-scale study of human cancers be made of the Livermore Valley and LLL by an independent group or agency.

(8) We found incomplete justification for the purpose of the Lab's continued involvement in lobbying for and designing nuclear weapons for the U.S. Why not more research in, say, energy alternatives?

(9) We found insufficient raw data throughout the report that would enable independent analysts to come to their own well-documented conclusions about detailed environmental impacts of the Lab and its activities. Furthermore, we have found evidence that any level of radiation can affect humans and is potentially harmful. We deem that the effects of radiation should be further examined by independent parties, as well as by the staff of LLL and that this information be provided to the public.

Upon further examination, the public and all these individuals potentially affected by any level of radiation should then determine the level of safety. We further recommend

that standards determined in this manner be the standards used to evaluate safe levels of radiation.

We feel that to do otherwise is to impinge upon our freedom to take responsibility for our own lives. Thank you for the opportunity to speak.

MR. FARMAKIDES: Thank you, sir. Mr. David Eller? Mr. Eller, would you kindly identify yourself, sir, for the record?

STATEMENT BY DAVID ELLER

MR. ELLER: Yes. David Eller, 3797 Oregon Way, Livermore. I moved to Livermore in 1958, and I've been here off and on, since. I do not work at the Rad Lab. I have not attended U.C. Berkeley, and I have no connection with U.C. Berkeley or LLL. I have been concerned about radioactive substances for several years.

First of all, Section 9.2 of the Draft states "Total or partial relocation. The cost of relocation would be extremely high, since new construction of facilities would be necessary, and personnel changes would involve much time and effort. The cost of maintaining the programs at another site would be at least comparable to those at the present site.

"Since the environmental impact from the Livermore operations is minimal, relocation is not a cost effective alternative.'

Well, the way I feel is, all radioactive -- anything to do with radioactive substances and materials at LLL should be transferred to either Los Alamos or Nevada or an area that does not have an earthquake problem or such a large population in close proximity. I feel threatened by the Lab. When we moved here in '58, atomic energy was considered hunky-dory. And I still have that seem feeling, that "Don't worry about it. Everything is fine."

Now on evacuation, Ken Miller spoke about calling up various agencies in the Bay Area and what they told him. The day after Three Mile Island I called up the local office of emergency services in Livermore and spoke with the assistant, and also the head of emergency services in Alameda County. And I was basically told that the reason there is no evacuation plan for Livermore and Alameda County is because there is no danger and because LLL has told them there is no reason for an evacuation plan.

So I talked with Mr. Jeff Garberson, the Public Information Officer at LLL, and he said that the only thing to worry about would be a discharge of tritium. And if it landed in a neighborhood, it would be such a small amount that there would be nothing to worry about and there would be no need for evacuation. And he said, anyway, the wind blows from the west, and the prevailing wind would blow it off to the east. And I said what happens if we get one of those rare northeast, easterly

winds during the summer, or at various other times which we do in the valley on occasion? He didn't say anything then.

So we have to depend, the citizens of Livermore and the Bay Area have to depend on an agency which says there is no need for an evacuation, there is no danger, so there is no evacuation plan. I don't really think it is the fault so much of our local emergency services or the Bay Area's or the state's, I think it is the fault of the industries, the various nuclear power plants and LLL, who have told them, forget it, there is no need for it. So I think we are going to have to be just a little bit more concerned and take it up on ourselves to just go ahead and have an evacuation plan even though there really isn't any danger, because I think that is probably what the residents of Harrisburg, Pennsylvania were told. Nothing can happen. Don't worry. There is no need for a serious evacuation plan.

I think that just about covers it, and I hope that the speakers that have spoken before me -- I haven't been here all day. But I've attended many public hearings on other subjects, water, air, pollution, et cetera. And I notice at those public hearings -- they may go on for hours and hours, people speak. And then I read the final EIS or see the outcome. There may be 20 speakers speaking on one thing. They make a very big point, but it never seems to really come out in the final except their letters. And I hope that this isn't just another public hearing,

and you decide to put a couple band aids on the Rad Lab out there, because that is what I am afraid is going to happen. A few band aids around the Rad Lab, and I don't think that really is going to solve the problem.

Thank you.

MR. FARMAKIDES: Mr. Eller, would you hold on, please?

Did you have any comments on the emergency evacuation plan, Mr. Du Val or Mr. Olsen in response to Mr. Eller's observation?

MR. OLSEN: I think the point is again this question of -- we have worked very closely with the City of Livermore in our mutual aid agreements, and in the county, and with DOE, with the California State Public Health in trying to be as ready as we can for any type of an accident that we have.

MR. ELLER: Mr. Olsen, isn't that basically for plutonium, if there was an accident out at the Livermore Airport?

MR. OLSEN: No, that's for any accident that occurs.

MR. ELLER: Well, I guess you had better tell Mr. Garberson, your Public Information Officer, that there are some other plans. Because I was told there was no need for any evacuation plan because there would never be an accident that would be of any magnitude that would make any reason to have an evacuation plan other than a plutonium accident, if a plane crash-landed at the airport. But, of course, we are told now that those containers are practically fail-safe, so we can

basically forget that. Mr. Garberson didn't tell me that. Those are my words. But your Public Information Officer said there was no need for any evacuation plans because there would never be an accident that would be great enough to be a reason for that. So I am glad to hear that.

But what is the evacuation plan? I would like to see it.

MR. OLSEN: Let me explain to you. Let me point out that as we've gone through the maximum credible accident, if you take the maximum credible accident that we have at the Laboratory, that we can postulate, there are a criticality accident; that is one of them. Now if that occurred, there would be a cloud pass over you as the closest resident. Now the dose that you would get would be in a short period of time. And your best action would be to stay in your house.

MR. ELLER: Would that depend on the speed of the wind?

MR. OLSEN: Any wind that you have -- the speeds of the wind can't change enough to make it significant because a cloud is going to pass over you in a short period of time. So your best --

MR. ELLER: That would depend on if it was one mile an hour wind or a --

MR. FARMAKIDES: Excuse me, sir.

MR. ELLER: -- or a thirty mile an hour wind.

MR. FARMAKIDES: Mr. Eller, if we are going to have a

discussion here, let's make it orally so we understand what each person is saying.

MR. ELLER: When I asked him, doesn't it depend on the speed of the wind, and --

MR. FARMAKIDES: Let him finish his point, then you can ask the question. As a matter of fact, we would prefer that you ask the questions of the Board. But you are the last speaker, so we'll relax that.

Mr. Olsen, could you kindly respond to that?

MR. OLSEN: You asked if it depends on the speed of the wind. Now if you have a cloud moving over you, the wind can only go so fast, and it can't vary a great deal. So, yes, it will depend on the speed of the wind, but that variation is small with respect to the time the cloud will pass over you. And what I am saying is that your dose to you, as an individual or people that are under the cloud, will be reduced by you staying in-house, inside more than it would be if you were moving.

Now evacuation doesn't really help you as far as dose is concerned. Now the maximum dose that you would receive is a few "r", a few rem. And, as people say, that is not good, because you are saying radiation is a hazard to you. But that is the amount that you receive, and evacuating would not assist that in any way.

MR. ELLER: I would probably receive the radiation

before I would evacuate, in other words?

MR. OLSEN: That's right.

MR. FARMAKIDES: Did you have any other questions?

MR. ELLER: Now I know the reason why there is no evacuation plan, because it would probably be too late to have an evacuation plan.

MR. DU VAL: Mr. Chairman, if I may make one brief additional comment.

I think the fact that the draft Environmental Statement concludes, in our judgment, that evacuation plans are not required for the accidents that are postulated should not, in my view, be construed as an endorsement that local communities, or a county or state should not have emergency planning for a variety of things and reasons that may come up. One need only to look in the current newspapers that railroad chemical accidents in Florida, tornadoes, certainly, the Three Mile Island incident regardless whether evacuation was justified, prudent -- our judgment at the time thought it was. Whether it is evacuation or other courses of action, I don't think any responsible individual would say to a community that they shouldn't have an emergency plan that considers various courses of action depending on the circumstances and the situation.

And I think there is a fine record, that has already been shown, of Livermore Laboratories being a good neighbor in assisting the city and the county in providing their resources

to the degree they can, and helping them developing that. The responsibility is on the community in terms of general planning. We would certainly encourage it, and we would certainly support it, and we'd hate to think anything we are saying is construed as saying we don't think a community should plan for the worst. That would be irresponsible.

MR. FARMAKIDES: Thank you. Thank you, Mr. Eller.

This completes then the list of limited participants unless someone has come in in the interim. I'll go over the names again. Wolfe, Sparks, Patterson, Brechin, Dougherty and Hughes.

Thank you very much, everyone. I think the Board has received your views and your comments throughout the day. At the moment we are trying to absorb at least a portion of them. We will have the transcript hopefully tonight or tomorrow morning, and we will go over the transcript. This Board will then sit down and begin to draft its report.

As we said earlier, we hope to have a report within 30 to 40 days. At that time you will see it, and you can judge us further.

Again, I would like to repeat one point that I made earlier. There are differences of opinion, obviously, between the various people that testified today, but I don't -- I do not find that there is anyone who has been manipulating the data that was in the Draft Environmental Impact Statement. I

don't think there has been any charge of that, any serious charge of that. The interpretations of data is something that varies.

We will assess your comments. We will assess your ideas and your views as to what are critical issues. And I daresay that we've already indicated to you that there are a number of critical issues that we have already focused on.

Is there anything else that you want to say?

For the staff, thank you very much for your comments and views here. I think that you have given some good advice to the public. And I think the public in turn has given good advice to you. With that, we will conclude this session. We will keep the record open to receive copies of those letters that we had earlier referred to. I think there are three of them.

Again, thank you very much, and good night.

(Whereupon, at 8:05 p.m., the public meeting was closed.)

STATEMENT OF ROLAND A. FINSTON, Ph.D. IN THE MATTER OF THE DRAFT ENVIRONMENTAL IMPACT STATEMENT, LIVERMORE SITE, LIVERMORE, CALIFORNIA

Hearing Held April 12, 1979, Livermore, CA

It is widely understood that α-emitting radioactive nuclides are among the most toxic because the high LET of these particles produces more biological injury per unit of absorbed dose than the β- and γ-emitters. The multiplicative factor (quality factor) for relative hazard produced by the α's is 20, according to the most recent recommendations of the ICRP[(1)]. Of the α-emitters, the most toxic are generally considered to the class of elements known as the actinides (Ac,Th, Pa,U, Np, Pu, Am, Cm..). According to a report prepared for the ICRP and adopted in 1972[(2)]:

> "The preponderance of data obtained for plutonium reflects the general consensus that this actinide is undoubtedly the most hazardous of the nuclides considered in this report".

Intravenous injection of ^{239}Pu-citrate in beagle dogs has resulted in bone sarcomas at the lowest dose level tested so far in which sufficient time has elapsed for expression of disease (0.016 μCi/kg). The dose-response appears to fit a straight line reasonably well. Hems and Mole state that the carcinogenic potential of injected ^{239}Pu in dogs is 10-20 times that of ^{226}Ra. The relative toxicity of ^{239}Pu to ^{226}Ra in adult man may be expected to be higher than in dogs[(3)].

In the dog, Park and Bair produced nearly 90 percent of bronchio-alveolar carcinomas after inhalation of 0.6 μCi of ^{239}Pu, corresponding to a lung dose of 2922 rad. Bair also analyzed the dose-effect relationship and showed that in the rat, an accumulated dose to the lung of as low as 10 rad might be carcinogenic[(4)].

In the matter of genetic effects of ^{239}Pu, experiments in male mice suggest that irradiation with α-particles is about 22 times as effective as chronic gamma irradiation for the induction of dominant lethals. ^{239}Pu has also been found to be highly effective in inducing translocation in male mice, even with very protracted exposures[(5)].

The present maximum permissible occupational body burden for plutonium is 0.04 μCi (0.64 μg). A respected health physicist has called for a reduction by a factor of from 10 to 240 (i.e. to 0.00272 μg - 0.064 μg) in the MPBB[6]. It is therefore quite important that the environmental impact of both routine and accident scenarios be fully evaluated when the facility under consideration is handling 10's to 100's of kilograms of plutonium (i.e. 10's of trillions of such maximum permissible burdens). I am deeply concerned that the draft EIS does not consider fully the consequences of earthquake-induced accidents at the Livermore Laboratory.

* * * * * * *

Roland A. Finston

REFERENCES

(1) Recommendations of the International Commission on Radiological Protection; ICRP Publication 26 para. (20). Annals of the ICRP, Vol. 1, No.3, 1977, Pergamon Press.

(2) The metabolism of Compounds of Plutonium and other Actinides. A Report prepared by a Task Group of Committee 2 of the International Commission on Radiological Protection. ICRP Publication 19, Chapter VIII, para. 13. Pergamon Press, 1972.

(3) United Nations Scientific Committee on the Effects of Atomic Radiation, 1977 Report to the General Assembly, with Annexes. Annex I, para. 261, p.611. United Nations, New York.

(4) U.N.S.C.E.A.R. Report, Annex I, para. 275, 276.

(5) U.N.S.C.E.A.R. Report, Annex H , para. 365, 371.

(6) Morgan, K.Z. Suggested Permissible Exposure to Plutonium and Other Transuranium Elements. American Industrial Hygiene Association Journal 36, 567-575, 1975.

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Tab 4

Staff Statement in Response to Comments

Received on the DEIS -- March 1979

STAFF STATEMENT IN RESPONSE TO COMMENTS RECEIVED

on the

DRAFT ENVIRONMENTAL IMPACT STATEMENT

LIVERMORE SITE

Livermore, California

(DOE/EIS-0028-D, September 1978)

March 1979

U.S. Department of Energy

Foreword

On September 18, 1979, the Department of Energy (DOE) issued for public review and comment a Draft Environmental Impact Statement (DEIS) which assessed the environmental impacts associated with current and continuing activities at the Lawrence Livermore and Sandia Livermore Laboratories. Comments were received from 23 individuals and organizations and in addition, numerous requests were made for a public hearing during the comment period. In response to the requests, DOE plans to hold a hearing on the DEIS at the Granada High School in Livermore on April 12, 1979. The hearing will afford further opportunity for public comment on the DEIS.

The major areas of concern raised in the written comments received on the DEIS include: (1) mission and location of the laboratories; (2) health effects and dose calculations; (3) seismology and hydrology; (4) emergency plans; (5) environmental monitoring analysis and standards; (6) accident analysis and central systems; (7) transport of radioactive materials in the environment; and (8) transportation of radioactive materials.

This Staff Statement was prepared in order to sharpen and focus the above areas of concern for discussion and examination at the hearing. The format includes a summary of the comments received and the proposed responses to these comments.

The Final Environmental Impact Statement will be issued later this year and will reflect the written comments received on the DEIS, as well as those presented at the public hearing.

The public hearing was announced in the Federal Register on March 2, 1979, (43 FR 11821). Details concerning the hearing are included in that notice. Requests to participate in the hearing should be made to Mr. W. H. Pennington, Department of Energy, Washington, DC 20545 (301 353-3034).

TABLE OF CONTENTS

1. Introduction

The Lawrence Livermore Laboratory (LLL) and Sandia Laboratories-Livermore (SLL) are located on adjacent sites about 65 km east of San Francisco in the Livermore Valley in southern Alameda County, approximately 5 km east of the City of Livermore. The sites occupy a combined area of 3.19 km^2. Open agricultural areas surround the Livermore operations. Site 300, a site located about 19 km southeast of Livermore is operated by LLL as a nonnuclear high explosives site. Hydrodynamic tests are conducted here in support of nuclear weapons development.

Nuclear weapons research and development has always been the primary mission of the Livermore operations. However, additional programs include magnetic fusion research, peaceful uses of nuclear explosives, bio-medical studies, laser fusion and laser isotope separation research. Most recently, programs to develop nonnuclear energy technologies have been established at Livermore.

The scope of the DEIS is limited to addressing the site specific environmental impacts of Livermore operations, alternatives to site operations and trade-off analyses between the cost of these environmental impacts and the benefits derived from continued operations. Environmental impacts include those from postulated maximum credible accidents associated with Livermore operations. The scope does not include assessing the environmental impacts of U.S. policy to produce or test nuclear weapons, nor of any other LLL/SLL activity conducted off the Livermore or Site 300 sites.

The notice of intent to prepare this Statement was placed in the Federal Register on July 16, 1976. Comments and suggestions were solicited. Only one letter comment was received in response to that notice and that one concerned traffic congestion on East Avenue. This issue was considered during preparation of the DEIS.

These staff comments were prepared in response to the 23 formal letters received from public review of the DEIS and will be used as background for the public hearing on the DEIS. Concerns noted by individuals, interested groups, and government agencies were placed in eight general categories as shown in the Foreword. The staff comments summarize the various categories, and provide information on proposed responses to these issues. Some letters asked specific questions, and in certain instances answers were provided in the staff response. However, some questions were beyond the scope of the DEIS.

2. Mission of the Laboratories

Comments: There were several comments requesting that the DEIS should evaluate the environmental effects of nuclear war, the design and testing of nuclear weapons, and be expanded so as to constitute an environmental impact statement for the U.S. Nuclear Weapons Program.

Response: The Livermore DEIS was prepared to provide input for decisions on the continued operation of DOE's Livermore Laboratories. Nuclear weapons research and development, which is conducted in support of U.S. nuclear weapons requirements, has been and continues to be the principal mission of Livermore operations. Requirements for nuclear weapons are imposed on DOE by Congress and the President as part of the overall national defense policy. The scope of the DEIS is limited to addressing the site specific environmental impacts of Livermore operations and does not include assessing the environmental impacts of U.S. policy to develop and test nuclear weapons.

U.S. defense policy and nuclear weapons requirements in support of that policy restrict alternatives as to DOE's weapons development activities. However, the converse is not true. DOE's development of nuclear weapons does not foreclose options with respect to the overall U.S. national defense program. Consequently, it is the DOE staff view that meaningful decisionmaking on the continued Livermore operations does not require a consideration of the issues associated with maintenance of a nuclear weapons stockpile or the possible environmental effects that might result in the event of a nuclear war.

3. Environmental Monitoring

Comments: There were comments that the statement contained insufficient information regarding the environmental monitoring program, specifically that not enough data were included for the reader to make an independent determination of the environmental impact of the Livermore operations.

Response: To provide the detailed information requested, information from the 1978 annual environmental monitoring report for the Livermore site will be included in the FEIS, together with information describing the sampling and analytical procedures employed in the program. The FEIS will also contain a summary of environmental monitoring data covering several previous years, so that the reader can evaluate the effectiveness of the effluent control measures at Livermore.

Comment: There was a comment that the DEIS fails to analyze the impact of routine and accidental releases on the following water supplies: Hetch Hetchy, South Bay Aqueduct, California Aqueduct, Patterson Reservoir and Del Valle Reservoir.

Response:

Hetch Hetchy: Water in the Hetch Hetchy Aqueduct flows in a pipeline located about 11 km southwest of the Livermore Site. Water is pumped from this aqueduct to provide the primary treated water supply for

the laboratories. As the source of water is in the Sierras, local effluents could have no credible effect on this supply. Quarterly samples of this supply are analyzed for radioactivity as a background measurement.

South Bay Aqueduct and Patterson Reservoir: The South Bay Aqueduct flows in a open channel at a closest distance of 1.6 km southeast of the Livermore Site. Upstream from this point water from this aqueduct is diverted to fill the Patterson Reservoir, located about 2 km northeast of LLL. Samples from the Patterson Reservoir are collected quarterly and are analyzed for gross alpha and beta radioactivity as well as tritium content. Accordingly, water from the South Bay Aqueduct is regularly monitored for radionuclide content at a point that is typically downwind from Livermore operations. The radioactivity observed in these samples is within the range of background.

California Aqueduct: The California Aqueduct, like the Delta Mendota Canal, is judged to be too distant from the Livermore Site to be influenced by operational releases of radionuclides ($\sim$15 km).

Del Valle Reservoir: This reservoir is sampled quarterly and analyzed for gross alpha and beta radioactivity and tritium content. Again, the radioactivity has been found to be within the range of background.

The above responses apply to operational releases. In the event of an accidental release, all surface water sources in a downwind direction from the point of release would be sampled.

Comment: There was a comment that the DEIS did not note that detectable levels of depleted uranium were discovered in soils in the Central Valley near Tracy, and that this uranium originated from Site 300 operations.

Response: The annual environmental monitoring report for 1973, URCL 51547, indicates that in many cases the uranium levels are elevated over that expected for soils in this area of California. Site 300,

in the Diablo Range west of the San Joaquin Valley, uses uranium, which is depleted in the ^{235}U isotope, in high explosive experiments. Since the $^{235}U/^{238}U$ ratios in the San Joaquin samples correspond to those for natural uranium rather than those for depleted uranium used in the explosive experiments, Site 300 is not considered a credible source for these elevated levels. This matter was not discussed in the the DEIS because it was concluded that the source of uranium in that area was due to uranium in the phosphate fertilizer applied to the soil. The presence of uranium in Florida phosphate rock, from which the phosphate fertilizer is derived, is generally recognized by agricultural authorities as posing little health hazard.

Comment: There was a comment that the DEIS should discuss the environmental impact of Site 300 operations on the approximately 100 acres of the site that have been released to the State as a wild life preserve.

Response: A radiological survey of this property was made in 1973 prior to the release and in compliance with AEC requirements for disposal of land. A terrestrial survey was made using a sensitive gamma detector (sodium iodide) with readings made at a nominal height of one meter above the soil surface. Soil samples were also collected for uranium using mass spectroscopy. The terrestrial radiation exposure rates, which vary from 4.0 to 6.5 microroentgens per hour (μR/h), are typical for natural background for soils in this area. Likewise, the variation of one to three parts per million in the total uranium is within the range observed in northern California.

Other impacts such as noise pollution from DOE high explosive testing are negligible due to the distance of the firing bunkers from the eastern site perimeter. The closest bunker, Bunker 812, which is located in a deep ravine, is nearly three quarters of a mile from the perimeter. Concentrations of airborne beryllium at sampling points between this bunker and the east site boundary are less than 1% of that specified as the permissible standard.

It was concluded from these measurements that Site 300 operations had no measurable impact on the area up to 1973. Since that time, annual surveys near the site perimeters have not indicated offsite impact. No construction is presently planned along the east perimeter.

Comments: Several comments questioned the practice of "whole soil sampling" for plutonium in soil measurements rather than using the "respirable dust techniques" employed by Dr. Carl Johnson in Colorado.

Response: While respirable particles of plutonium in air are, of course, the particles most likely to be retained in the respiratory tract of man if inhaled, the procedures by means of which the relationship of particles in soil to particles in air is determined remains a very difficult and uncertain one despite extensive research. For this reason, until recently all radiation protection organizations issued exposure limits and air concentration limits in preference to issuing soil limits in order to regulate and limit inhalation exposure.

Dr. Johnson's method of soil analysis has not been accepted because there is no background information to allow estimates of air concentration from these measurements. Further, it is doubtful that the methods are reproducible because the quantity of fine particles will vary depending upon soil conditions and topography; wind resuspension of soil is a complex process governed by far more than the presence of small particles on the soil surface.

Consequently, Dr. Johnson's method, which consists of sweeping the soil surface, has been criticized because the technique is not reproducible with respect to the depth of the sample, and because the physical and chemical treatment following collection materially alters the particle size distribution from that found in the soil. Moreover, by failing to measure plutonium beneath the surface, the Johnson technique overlooks possible uptake by vegetation with subsequent environmental dispersion and/or transport.

At Livermore, soil samples are collected in the offsite vicinity of the site as part of environmental surveillance monitoring. Here, there is a requirement to collect the sample to sufficient depth to account for essentially all the plutonium that may be present. A surface swept sample offers no such assurance. The sampling method used at the Livermore Site is the same as that recommended by the Nuclear Regulatory Commission (NRC) Regulatory Guide 4.5, which specifies a surface sample collected to a depth of 5 centimeters. The Environmental Protection Agency (EPA), in their "Proposed Guidance on Dose Limits for Persons Exposed to Transuranic Elements in the General Environment" (EPA 520/4-77-016), recommends that soil samples of one centimeter depth be taken when providing evidence of compliance with the guidance. However, in a recent EPA comparison study (D. E. Bernhardt, J. D. Bliss, and G. G. Eadie, "Comparison of Soil Sampling Techniques at Rocky Flats," Selected Environmental Plutonium Research Reports of the NAEG, DOE/NVO-192 - in press), EPA stated that 0 to 5 centimeter samples can be used to conservatively estimate the activity per unit area for the upper one centimeter.

4. Geology and Seismicity

Comments: There were a number of comments regarding the definition of the potential geologic and seismic hazards to the LLL/SLL sites. These comments can be conveniently grouped under the following topics:

(1) The tectonic model used in the DEIS is incorrect.

(2) The potential for surface faulting at the LLL/SLL sites was not properly evaluated.

(3) The seismic hazard was not properly evaluated.

A number of specific issues were raised with respect to each of the above categories. These specific issues will be discussed in the appropriate section of the staff's response.

Response: The recent publication by Herd[1] and the extensive field investigations at the Vallecitos site have been reviewed. In light of the new information noted from these reviews, several areas in the original investigations[2-4] have been identified as requiring additional field investigations. For the reasons discussed below, these additional investigations probably will not change the basic conclusions given in section 2.3.3 with respect to the safety of the LLL/SLL sites and the adequacy of the design of the critical facilities. In order to confirm this conclusion, an extensive program to reassess the potential seismic and geologic hazards to the LLL and SLL sites has been developed. This program is described below in the section "Proposed and Ongoing Investigations."

Although References 2-5 reflect the bases for current understandings of the local seismology, Ref. (1) and the ongoing investigations at Vallecitos also were reviewed to determine what impact this information would have relative to the safety of the LLL/SLL sites. The potential impact could be in two areas; (1) change the specification of the ground motion for the Safe Shutdown Earthquake (SSE) and (2) alter the potential for surface faulting.

The ground motion specified for evaluation of our critical facilities is based on placing an earthquake on the Tesla Fault at the nearest approach to the site boundary. The mapped length of the Las Positas Fault is approximately 15 km (Ref. (1)). The estimated length of the Tesla Fault in Refs. 2-5 is about 30 km, thus the potential maximum earthquake on the Las Positas Fault would be smaller than on the Tesla Fault. Thus the "discovery" of the Las Positas Fault does not have any impact on the size of the SSE, nor does it bring the earthquake any closer. In fact, it should be noted that Fig. 4 of Ref. 2 shows a possible fault located which subsequently has been identified by Herd[1] as the Las Positas Fault. This possible fault was not considered in detail in Ref. 2 because the Tesla Fault was clearly more significant and was inferred to pass under the LLL/SLL sites. In response to one comment, it should be noted that even if the Las Positas and Verona fault systems are considered to be the single tectonic system (which is extremely improbable) the total length of this combined system would be no longer than the very conservatively assumed length for the Tesla Fault.

While the extent and importance of the Las Positas fault system to the tectonic model of the Valley was somewhat unexpected, as discussed above, it does not impact the design basis.

The basis for the statements that the critical facilities do not have to be designed for surface faulting are based on the field work reported in Refs. 3 and 5. Additional discussion is provided later.

Tectonic Model The tectonic model used in Refs. 2-4 is clearly in need of updating as a result of Ref. 1 and the large amount of information generated at the Vallecitos site. The model was based on the reasonably well established regional tectonic fabric of the area with a general northwest/southeast trend. This includes the more major structures like th Calaveras, Greenville and other possible faults such as the Mocho and Livermore faults. The model did not include the Verona and Las Positas fault systems. The Verona fault is a

dip slip feature related to what appears to be a result of east-west compression of the Livermore Valley. The Las Positas fault is an anomolous feature trending northeast/southwest which is difficult to explain tectonically in the regional framework.

Because of close proximity of the Las Positas fault zone to the LLL/SLL sites and (at least in some tectonic models of the Valley) its importance to the tectonics of the Valley several questions about this unique transverse feature do arise which require investigation. For example:

1. Why does the Las Positas fault zone trend normal to the regional structural fabric of the region?
2. Is it active, what is the gross amount of displacement, what is the dip component (sense of movement) and is there creep associated with the feature?
3. What is the relationship of the Las Positas fault with other known faults in the area (e.g., Calaveras, Greenville, Verona and Livermore)?
4. What is the extent of the fault - does it terminate, merge, truncate or continue?
5. Could it be nontectonic in origin such as a compaction fault (gravity feature)?

LLL has been involved in the review of the Vallecitos site under contract to the U.S. Nuclear Regulatory Commission. As part of the NRC effort, LLL has directed several studies[6,7] of the Vallecitos site. A small part of that effort was to briefly synthesize all the available information and examine possible tectonic models of the Livermore Valley. A number of possible models exist, several of which would reduce the seismic hazard at the LLL/SLL sites relative to the seismic hazard defined in Refs. 2-4 and the DEIS. For example, one model suggested in reference 7 is that apparently stress is being transferred across the Livermore Valley (east-west) perhaps by the Las Positas fault or any other unknown transverse fault as well as folding. The sense or movement on the Las Positas is predominantly

vertical (normal movement) with the northside down dropped. If the valley was rotating then the the Las Positas may not be tectonic or it may be a secondary fault only capable of sympathetic movement and further, it may not be capable of generating its own major earthquake.

Based on known folds and faults in the Livermore region, the valley is probably under compression, being squeezed between movement along the Calaveras and Greenville faults, and the predominant stress from the west. Further, based on the regional Bouguer gravity map a pronounced anomally trends along the southern boundary of the valley. This is interpreted as a major basement discontinuity and may reflect the north end of the Diablo Antiform (north side down dropped).

Assuming the principal compressional stress is from the west end a buttressing effect persists parallel to the Greenville/Altamount Highlands and another buttressing effect trends along the north end of the Diablo Antiform/Las Positas fault then a simple structural model can be developed. It should be pointed out that other gravity gradients parallel the Calaveras and Greenville faults. The thick sedimentary sequence in the Livermore Valley, some 8,000 feet thick, has been folded into a broad syncline. These sediments are inferred to be Plio-Pleistocene age and due to their great thickness, downwarping caused by compressional folding is suggested.

If this model is anywhere near correct, then the Las Positas fault is the buttress boundary along which active (?) north-south folding is taking place to the north (Livermore Valley). South of the fault plane the competency of the basement rock is far greater than the sediments to the north. Therefore, the compressional shortening effect is not as great. In short, differential shortening west to east is taking place either side of the feature. The shortening

north of the fault is causing downwarping resulting in the downdropping of Livermore Valley and subsequent valley filling. Regional uplift of the Diablo Antiform south of the Las Positas fault also may be the result of this shortening effect.

This is perhaps the most simple/obvious model - others have been developed. What is interesting about this model is that the Las Positas Fault does not have to be considered tectonic though it indirectly reacts to tectonism-folding. It is merely a zone of weakness along which folding takes place.

A possible model at the other extreme is to consider the Las Positas fault zone as a major tectonic fault capable of generating a significant earthquake based on its length. As discussed earlier, this should not alter the assessment of the seismic hazard because the seismic hazard was based on a fault which was conservatively taken to be longer than the Las Positas fault zone.

The proposed LLL investigations coupled with the information from the investigations at the Vallecitos site should resolve the important questions about the appropriate tectonic model for the Livermore Valley.

Potential for Surface Faulting A number of comments have been made about the adquacy of the basis for the statement that the Livermore Site critical facilities did not have to be designed for surface faulting. Staff has been concerned with this problem since the inception of the seismic safety review program. For example, one of the main recommendations made in Ref. 2 was that a field investigation be conducted to determine if there was a potential for surface faulting at the LLL site. The results of this investigation are reported in Ref. 3. A number of different methods were used to attempt to locate the various faults. None of the faults could definitively be located. The fault traces shown on Fig. 9 of Appendix 2A of the DEIS are all conservative interpretations of the field work

and other tenuous evidence. It should be noted that Herd's[1] map does not show any faulting under the LLL site and only the Las Positas Fault passing through the SLL site.

A question has been raised about the truncation of several faults near the LLL/SLL site boundary on Fig. 9 of Appendix 2A. The reasons for truncation are given in Ref. 3. The various methods used to locate possible concealed faults failed to show any evidence for those faults extending under the LLL/SLL sites. Herd's[1] work would provide an alternative explanation. As noted above, the faults shown on Fig. 9 are all concealed faults and the basis for extending these faults was inconclusive. Because the extension and location of these faults was made on (from the point-of-view-of-safety) very conservative grounds, and in light of Herd's[1] work, it is possible that none of these faults exist on the LLL site.

Assuming that Herd's[1] map is correct then the only potential for surface faulting at the LLL/SLL sites is from the Las Positas fault system. It is unlikely that the faulting from the Las Positas fault zone would extend under the critical facilities at LLL as they are outside the extremely conservative 1/2 mile control width used for site nuclear power reactors. The Tritium Research Facility on the SLL site is within the control zone of the Las Positas fault system. For this reason, additional field studies were taken to investigate the existence of the Las Positas Fault, as well as, splays of the Tesla Fault in the vicinity of the Tritium Research Facility on the SLL site. The results of these investigations are reported in Ref. 5. A number of different methods were used to attempt to locate any faulting in the vicinity of the Tritium Research Facility including trenching. No evidence was found by trenching or during the surface geologic examinations or in any other phase of the investigation to indicate a possible hazard to the facility due to surface fault rupture along an active fault.[5]

As indicated above, although it is unlikely that the Las Positas fault system would extend under the LLL critical facilities, the field investigations carried out and reported in Ref. 3 were not designed to locate faults on strike with the Las Positas fault system. This is one of the areas which require additional investigation. The proposed program (discussed later) will determine the full width of the Las Positas fault zone in the vicinity of the LLL/SLL sites and any other potential fault under or near the critical facilities on the LLL/SLL sites.

Adequacy of the Seismic Hazard A number of comments have been made questioning the adequacy of the definition of the seismic hazard given in section 2.3.3 of the DEIS. The questions primarily dealt with the difference between the 0.8 g acceleration value used to anchor the spectra given in Ref. 2 and the 0.5 g given in Ref. 3, 4, and 5. Several comments were also received to the effect that the same specification of the seismic hazard at the LLL/SLL sites as was given by US NRC for the reactor at the Vallecitos site should be used.

It is unduly conservative to use the same definition of the seismic hazard for evaluation of the critical facilities at the LLL/SLL sites as for the reactor at the Vallecitos site for several reasons. First, and most important, the seismic hazard for the LLL/SLL sites is much lower than for the Vallecitos site which is located very near to (less than 1-1/2 miles) one of the major splays of the San Andreas fault system and on the Verona fault system. The level of seismic activity is much lower for the Greenville, Tesla, and Las Positas fault systems than for the Calaveras and Verona fault systems. Also, a much larger earthquake could occur on the Calaveras fault system than on any of the other fault systems of the Livermore Valley. This larger earthquake was the basis for the 1 g value assigned as the peak free-field ground motion at the Vallecitos site.

Secondly, nuclear power reactor criteria was used to specify the hazard for the General Electric Test Reactor (GETR) at the Vallecitos site. The hazard to the public from the critical facilities at the LLL/SLL sites is significantly lower than from nuclear power reactors.

Lastly, it should be noted that there are no clearly defined standards that apply to facilities similar to those at LLL/SLL. NRC is currently using a risk approach to evaluate existing commercial plutonium processing or fuel fabrication facilities rather than using power reactor criteria to evaluate such facilities.[9] The approach in evaluating the seismic hazard at the LLL/SLL sites in the DEIS is consistent with the approach used by NRC.[6,8]

The difference between acceleration values (0.8 g vs 0.5 g) used to anchor the response spectra in Refs. 2-5 can be traced to a different level of conservatism in each report. The g value in Ref. 2 was based, inpart, on the assumption that up to 1/2 of the total fault length could rupture whereas in Ref. 3 it was assumed that an appropriate level of conservatism would be to assume only 1/3 of the total length of the fault could rupture (the Tesla Fault is estimated tobe from 26 to 34 km long). A number of other judgments must also be made to translate the possible rupture length into ground motion at the LLL/SLL sites. A number of these judgments are given in Refs. 2 and 4. There is no generally agreed methodology available to define the ground motion in the near-field of a significant earthquake at a soil site such as LLL/SLL sites.[9,10] In view of many problems and judgments that must be used with the so called deterministic approach used in Refs. 2-5 staff believes that a much better approach is to systematically develop the seismic hazard for the LLL/SLL sites using an approach similar to the approach used by NRC in Ref. 6 for commercial plutonium facilities. In this approach, the various parameters are allowed to range over all physically

possible values; however, the frequency or likelihood of occurrence is also an important parameter. The proposed program, discussed below, is geared to the proper assessment of the seismic hazard at the LLL/SLL sites.

Proposed and Ongoing Investigations As indicated in the above sections, the recent work by Herd[1] and the ongoing investigations at the Vallecitos site have required reassessment of the previous work in Ref 2-5. It was indicated that some additional investigations would be useful to resolve any questions with regard to the potential for surface faulting. It was also noted that Livermore staff has been involved with the ongoing work at the Vallecitos site and has continually updated the overall analysis of the safety of the SLL/LLL site and facilities. Staff is in the process of developing a program to reassess the conclusions given in the DEIS.

This program will include the following steps:

- o Synthesize the data from Herd, Refs. 3 and 5, and the Vallecitos site studies to formulate a new tectonic model for the Livermore Valley consistent with the known facts.
- o Use this new model and establish a good field investigation study. The goals of this study are to resolve all questions with regard to the potential for surface faulting at the LLL/SLL sites and to develop activity rates for the active faults in the Livermore Valley in the vicinity of the LLL/SLL sites.
- o Perform a detailed probabilistic based seismic hazard analysis for the Livermore site using appropriate activity rates for the Las Positas-Verona fault systems developed from field investigations.

The program is not finalized at this time. It will include the following investigations:

- o Geologic investigation of near surface features visible in trenches to be excavated on the LLL site where airphoto lineaments, or the extension of such features, have been detected in the first study.

- Resistivity studies to determine depth of ground water table and for locating zones of faulting since such zones are ground water barriers and are, thus, highly visible using resistivity methods.
- High resolution seismic survey of Livermore site and surrounding region for location of faults.
- Installation of a seismic network in and around the Livermore Valley for monitoring microearthquake activity on currently active faults within the valley.

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10. D. L. Bernreuter, "A Geophysical Assessment of Near-Field Ground Motion and the Implications For the Design of Nuclear Installations", Proc. of the Specialist Meeting on the 1976 Friuli Earthquake and the Antiseismic Design of Nuclear Installations. CNEN, Roma, 1978.

5. Waste Management

Comment: Some reviewers were critical of the DEIS's omission of the accidental dumping of americium waste at the Eastern Alameda County Disposal Site.

Response: This incident occurred in the latter part of August 1978, while the DEIS was in the process of being printed. The FEIS will describe this accidental disposal and subsequent retrieval of americium.

Comment: Onsite radioactive waste storage at LLL and ultimate shipment offsite were concerns expressed since the DEIS did not address the environmental impacts and hazards of such operations.

Response: All storage of radioactive waste is confined to the liquid waste processing areas, radioactive materials handling areas, and the solid waste disposal yard where the latter waste materials are sealed in Department of Transportation (DOT) approved containers. The potential offsite impact of this operation was considered along with a number of other operations with radioactive materials, but only those accident scenarios with the worst possible consequences were shown in the DEIS. The offsite consequences from all other operations were less than those presented in the DEIS, hence, no impacts were shown specifically for the storage of solid radioactive wastes.

Once the materials leave the Livermore Site in accordance with DOT and DOE regulations, the responsibility for safety is transferred to the carrier and to the waste management facility.

Comment: One letter of inquiry showed a concern for past, present, and future burials of waste at Site 300 with emphasis on the possible need for environmental impact analysis.

Response: The practice of burying soil, gravel and other test debris at Site-300 is governed by the rules in Title 10, Part 20 (paragraph 20.34) of the Code of Federal Regulations, as noted in Section 3.5.6.1 in the DEIS. The impact of this disposal technique has been and will continue to be closely scrutinized by the Site 300 environmental monitoring program, especially through air and groundwater surveillance. Air and groundwater surveillance data for Site 300 will be presented in the FEIS.

Comment: The comment was made that there was insufficient information on toxic chemical waste management and disposal, especially with regard to quantities released into the environs.

Response: Because of the diversity in the number of chemicals handled at LLL and SLL, their disposal must be discussed in a generic sense, as was presented in Section 3.5.3 of the DEIS. However, the FEIS will be more specific about the types and quantities of chemicals handled.

Comment: There was a question regarding the length of time to alert state and county authorities when accidents result in offsite contamination.

Response: DOE and State of California regulations require that appropriate notifications be made for different classes of accidents. The notification times vary, depending on the seriousness or expected consequences of the event. Both LLL and SLL adhere to these regulations and will continue to do so in the future. The only changes would be to meet new criteria as regulations are revised.

Comment: There was a question about the size of particles collected by the HEPA filters and why there was no evidence of particle size analysis of stack effluents in the DEIS.

Response: The efficiency of the HEPA filters used at LLL/SLL has been evaluated by researchers at Los Alamos Scientific Laboratory and others. (References: Harry Ettinger, J. C. Elder, and M. Tillery, "Performance of Multiple HEPA Filters Against Plutonium Aerosols," LA-5784-PR, Los Alamos Scientific Laboratory, November 1974, and B. Schuster, T. Kyle, and D. Osetek, "Multiple HEPA Filter Test Methods," LA-6852-PR, Los Alamos Scientific Laboratory, June 1977.) The efficiency of HEPA filters is shown to increase when the size of impinging particles is larger or smaller than 0.3 microns (0.3 μm). This size (0.3 microns) is a minimum efficiency point in the curve denoting efficiency versus particle size. Tests show that the efficiency of these filters for removing particles of 0.3 micron diameter is 99.97%. For particles larger or smaller than this critical size the efficiency is greater than 99.97%. (References: N. A. Fuchs, Mechanics of Aerosols, Pergamon Press; S. K. Friedlander, Smoke, Dusts, and Haze, Wiley and Sons, New York, 1977; R. D. Cadle, The Measurement of Dust Particles, Wiley and Sons, New York, 1975.)

There is no practical way to quantify particle sizes, since the number of particles actually collected is not sufficient to provide reasonable statistical validity. However, stack effluents following HEPA filteration are monitored routinely, to demonstrate the effectiveness of filter performance.

Comment: There was a comment that the health consequences of radon 222 emanating from depleted uranium waste buried at Site 300 should have been addressed.

Response: Depleted uranium is a by-product in the process by which enriched uranium (enriched in ^{235}U) is produced from natural uranium. In a preliminary step, the uranium decay products, including radium are chemically separated from the uranium. As a result, the depleted uranium used at Site 300 is essentially free of radium 226 (the parent of radon 222).

Comment: Several questions were raised with respect to the influence of earthquakes on HEPA filters.

Response: All critical facilities at the Livermore Site have been analyzed for their ability to withstand peak ground acceleration equivalent to 0.5g. A site-wide equipment tie down program was also carried out to afford protection against damage to glove boxes and filter systems. All systems but one (Bldg. 251 which is being upgraded) can withstand such an earthquake. The question of cumulative damage within Bldg. 251 does not apply because nearly all of the "in process" radioactivity is contained in one glove box. As a result, even in multiple glove box damage, the consequences do not exceed those of the maximum credible accident.

6. Accident Analysis and Dose Assessment

Comment: There were comments asking if farmers had been notified of plans for possible milk replacement following a criticality accident. It was also noted that milk replacement should not be depended on to reduce population dose.

Response: The forage-cow-milk pathway is not a significant population dose pathway since no dairies are located near the Livermore site. The present plan is to survey nearby farms and ranches in the event of a criticality accident and to replace potentially contaminated milk.

Comment: One commenter asked that dose commitments, rather than annual doses, be used to measure the effects of postulated accidents and routine releases.

Response: The total dose from postulated accidents is addressed in the DEIS, Section 3.9. For routine releases, annual doses are found in the monitoring report, and relevant information from that report will be included in the FEIS. For the fission products released in a criticality accident, or the tritium from a tritium release or the other routine effluents, the total lung dose is received over a few weeks or months. For the ^{244}Cm release, the total dose is essentially received in seven years.

Comment: The size of the source terms and the methodology used in calculating routine and accident doses were questioned.

Response: It is believed that most accidents will occur from human error. Therefore, the tritium and curium release quantities are fixed at the largest amount that will be handled at one time. The size of the criticality accident is set at a credible maximum because the very process of uncontrolled chain reaction changes the geometry of an accidental critical assembly and makes it sub-critical.

The quantity of radioactivity involved in a hypothetical accident is multipled by an atmospheric diffusion parameter (X/Q) to obtain the concentration of each radionuclide in the air.

The air concentration is multipled by the time of release and a pathway-specific dose conversion constant to arrive at the dose a person would receive if he stood directly under the radioactive cloud during its passage or drank milk from a cow that had grazed on a pasture directly under the cloud.

The literature references for criticality size and dose conversion constants can be found in the DEIS. The process of selecting the appropriate X/Q was not well explained in the DEIS and will be better documented in the FEIS. A summary explanation follows.

Wind speed, wind direction, and atmospheric stability were tabulated every 1/2 hour for one year - using data taken from a meteorological tower at LLL. These data are used by a computer program to calculate the average and 95 percentile worst X/Q's for 16 compass directions at distances from 100 meters to 100 kilometers under the assumption of Gaussian diffusion.

Differences exist in the doses presented in the DEIS and doses calculated earlier for similar releases. The earlier calculations assumed a statistical distribution of diffusion constants which is not supported by the data collected at the LLL site. The actual data was used for the DEIS.

For accident dose calculations, the atmospheric diffusions during east winds are used. This wind direction allows the largest population dose. As explained in the DEIS, local topography limits the concentration of deposited radioactivity in the San Joaquin Valley. Thus, the major environmental impact is postulated to be the dose to people west of the Livermore Site.

For routine effluent releases, the doses are calculated using the average X/Q's mentioned above, weighted by the wind frequency. This allows calculation of doses to persons at any point around the laboratories' perimeter, - under the assumptions of continuous release and continuous residence at a given point.

For $^{15}O_2$ $^{13}N_2$ and ^{41}A effluents inhalation and submersion pathways are both considered. For tritium complete conversion to water (HTO) is assumed - so that inhalation and skin absorption pathways are considered. Population doses were calculated using the population distribution in the DEIS. The accidental or effluent concentration at the center of each of the sector segments shown in the DEIS (Fig. 2-12 and 2-13) was multipled by a dose conversion constant and by the population in that sector segment. For accident population dose for each sector segment west of LLL-SLL was summed to produce Table 3-17 of the DEIS. For effluent releases, population doses from sector segments in all directions are summed.

Comment: One comment noted that the fenceline accident dose limits of NRC's regulation, 10 CFR 100, apply only to reactors.

Response: The reviewer is correct. Comparison of the postulated accident doses with 10 CFR 100 limits was made to provide perspective only. There is no guideline for non-reactor facilities.

Comment: One commenter wanted information on the X/Q at the nearest residence, school, and pastured cow.

Response: For residence and school, these numbers can be adequately approximated by using distances scaled from Fig. 2-11 of the DEIS and interpolating the X/Q's found in Tables 3-3 and 3-8 of the DEIS. The nearest commercial milk cows are in the San Joaquin Valley so that X/Q's will be lower than any given in Tables 3-3 and 3-8.

Pastured milk cows in the Livermore Valley provide milk for single families. These cows are few in number and their number and location changes frequently. Therefore, no meaningful response can be made to the request for this specific X/Q.

Comment: A reviewer questioned the assertion, on page 3-65 of the DEIS, that radioiodine inhalation doses are not increased by the presence of fog since the droplets are too large to be inhaled.

Response: The assertion in the DEIS is incorrect. Literature references give a mean diameter of fog droplets around 12-14 micrometers (μm). Since 10 μm is the upper bound of particles inhaled into the deep lung, those fog droplets smaller than the mean can be inhaled into the deep lung. This and other dose pathways will be further treated in the FEIS.

Comment: There was a question regarding the criteria used in categorizing the accidents in Appendix 3C as having "offsite significance," and the doses received by members of the public from these accidents.

Response: As used in Appendix 3C, "Offsite significance" refers to radioactive material outside the site boundary, discernably above natural background or release of any radioactive or chemical materials to the sewer that causes diversion or interference with normal operations at the Livermore Water Reclamation Plant.

The hazards assessment of the March 26, 1963, criticality was published in Health Physics, Vol. 10, 1964 pp 183-192. The maximum radionuclide concentration found at the site perimeter was an air sample containing 4 times the maximum permissible continuous concentration of ^{131}I. Soil, vegetation, and milk samples were collected following the accident; all were found to be at approximately natural background levels.

Assessment efforts following the August 6, 1970, tritium release are documented in "Tritium", edited by Moghissi and Carter, TID document number CONF-710809, (1973) pp 611-622. Air, water, vegetation, and milk samples were measured for tritium. The maximum dose from cloud passage was estimated at less than 0.025 mrem. No tritium was found in water or milk above background levels.

Assessment of the consequences of the May 1967 release of ^{239}Pu to the sanitary sewer is summarized in Appendix 3C of the DEIS and a literature reference is given.

Section 3C will be updated to contain further pertinent information.

Comment: One reviewer asked for an explanation of the phrase "appropriate radiation or concentration guides" as used in the DEIS.

Response: International and national radiation protection organizations have for many years provided, and continue to review and provide, radiation protection recommendations to governments and to the responsible Federal agencies, respectively. These recommendations were adopted by the former Federal Radiation Council (whose authority now has been transferred to EPA) and were incorporated into operating procedures of the affected Federal agencies. The recommendations and guides include radiation exposure limits for individuals and for populations, including limits on the concentration of radionuclides in air and in water. Specific guides are found in the Department of Energy's Immediate Management Directive 0524 "Standards for Radiation Protection," and are essentially the same as the radiation and concentration guides in the Code of Federal Regulation 10, Part 20, "Standards for Protection Against Radiation."

7. <u>Emergency Response Plans</u>

<u>Comment</u>: The Emergency Response plans were questioned since they were limited to onsite procedures.

<u>Response</u>: Limiting the discussion to onsite procedures was not intended to imply that DOE plans are not coordinated with those of local or state agencies for incidents having the potential for offsite consequences. DOE assesses potential incidents at the Livermore site that might affect the public and coordinates its planning with appropriate agencies having the authority for the protection of public health and safety. Under the DOE Radiological Assistance Plan, DOE makes available its resources, such as personnel, equipment, facilities, data acquisition network, etc., to such state and local authorities. DOE is in agreement with the State Health Department, Radiologic Health Section relative to the notification and response procedures concerning radiological incidents occurring in California. Accordingly, in the event of radiological incident that may affect the offsite public, DOE will notify the State Radiologic Health Section via the State Office of Emergency Services 24-hour telephone station in Sacramento. DOE will coordinate the deployment of its resources at the incident scene in support to the local agency in charge. DOE's radiological assistance does not in any way abridge state or local authority, but works in cooperation with state/local officials in radiological emergency operations.

<u>Comment</u>: There was a request that notification procedures be summarized for those organizations with whom emergency action agreements exist.

<u>Response</u>: LLL has mutual aid agreements with the cities of Livermore, Pleasanton, and the Alameda County. A mutual aid agreement also exists between the Livermore Site and Valley Memorial Hospital. Laboratory emergency forces are prepared to notify local and county officials if an emergency requires offsite actions. The LLL emergency dispatch center has a number of communication modes

connecting it with local emergency centers. These include the Radio Mutual Aid Frequency with Livermore and Pleasanton fire departments and Alameda County emergency control center and the microwave telephone which connects LLL with all other emergency dispatch centers in the area.

8. Health Effects

Comment: There was a comment that the DEIS should address the fact that fourteen LLL employees have contracted melanoma and should point out that there may be a relationship between the incidence of this disease and Laboratory operations.

Response: As a first step, LLL has supplied the California Department of Health Services with employee records covering a period of approximately ten years. These data are being used as part of an epidemiological study to determine if the incidence of melanoma experienced by LLL workers is statistically different from that observed in similar age groups in surrounding counties.

Comment: The question was also raised as why the State Office of Health Services has not been encouraged to complete their melanoma study.

Response: In 1978 the Public Health Department requested temporary support for a computer programmer working on the melanoma project and this support was provided. Additional support will be provided as necessary to complete the study.

Comment: Several reviewers commented that the DEIS did not demonstrate regard for the health and safety of employees.

Response: Providing for health and safety of employees is a requirement specified in contracts for operation of DOE facilities. At Livermore, experts representing all safety disciplines provide safety guidance to both employees and management in planning, establishing, and maintaining a low-risk work environment. Safety teams monitor

all Livermore operations to detect and evaluate hazards. Emergency response personnel are trained to control accidents or emergencies. Research is conducted in such areas as fire safety, radiation detection and protection, chemical hazards, and respiratory protection. During the history of the Livermore Site, there have been four fatal job-related injuries. All of these resulted from vehicle accidents, three which occurred offsite. Falls or falling objects striking personnel are the most frequent cause of lost time accidents. Less than one percent of the lost time injuries are caused by toxic materials or radiation.

The beryllium monitoring program is typical of the Livermore Sites' attention to possible exposure to hazardous chemicals. Continuously operating air samplers provide filter samples from which the atmospheric working environment of all areas handling beryllium is evaluated. Swipe samples are also periodically collected from these areas to minimize surface contamination as a source of airborne beryllium.

Laboratory programs involve the use of a wide variety of radioactive and radiation producing equipment. LLL issues radiation dosimeters to all employees and to visitors who may enter specified buildings. These dosimeters record both natural background radiation and any occupational radiation to which the employee was exposed. Subtractions are made of the average exposures received from natural sources.

During 1978, which was typical of the past several years, the 7,965 Livermore Site employees had a radiation exposure distribution as follows:

Employees, %	Whole body dose, (mrem)
87.8	none detectable above background
8.5	<50
1.7	50-99
1.7	100-499
0.3	500-2999
0.0	>3000

For purposes of perspective, the maximum permissible whole-body dose for radiation workers is 5000 mrem/year. In the Livermore area, the natural background radiation dose from cosmic rays, terrestrial radiation, and radionuclides in the body is about 100 mrem per year. The above table shows that less than 2% of Livermore employees received an occupational exposure in excess of that received from natural sources.

In addition to the personnel dosimetry program, the Livermore Site maintains extensive workplace monitoring in those facilities in which radionuclides are handled. This program involves routine radiation surveys of work areas, swipe checks of work surfaces, hand and foot checks prior to leaving radioactive materials handling areas, and the collection and analysis of over 20,000 air samples per year to monitor for radionuclides in the atmosphere. In addition to monitoring the workplace, more than 2,500 bioassay (urine and fecal) samples are collected from employees who work in radioactive materials handling areas.

Finally, over 500 whole-body counts are made on employees per year. Whole-body counting is a standard technique using sensitive radiation detectors to directly monitor for radiosotopes in the body.

The bioassay sampling and whole-body counting programs are conducted to insure the effectiveness of radioactive materials handling and containment procedures and to assess routine or accidental uptakes, if any, that might have occurred. The vast majority of the bioassay samples and whole-body counts do not indicate any radioactivity above background levels. During 1978, which was a typical year, there were no uptakes of radioactive material which exceeded permissible guidelines.

9. Transportation

Comment: There was a question asking if the Livermore Site planned to resume routine air transport of plutonium if and when the container tests were satisfactorily completed.

Response: A container developed for the Nuclear Regulatory Commission and DOE has been approved for use in shipping plutonium by air. This container was evaluated by the Aeronautics and Space Engineering Board of the National Research Council. The Council issued a certificate of Compliance No. 0361, dated September 5, 1978. This certificate has an expiration date of September 30, 1983. However, at present, the Livermore Site has no plans for routinely shipping plutonium by air. If such shipments are made, they will be governed by the conditions set forth on pp 3-70 and 3-71 in the DEIS.

Comment: One commenter asked if there had been any nonroutine flights of plutonium in the last 18 months.

Response: There has been only one such shipment to date. Several small plutonium sources totally less than 40 microcuries were shipped by air to England for calibration of medical equipment. Approval for this shipment was obtained from DOE Headquarters.

Comment: A commenter asked why LLL did not search for evidence of low-level radiation emissions from trucks carrying radionuclide to and from the site.

Response: All radionuclide containers are monitored for evidence of loss of containment using swipe testing. In addition, the truck carrying the material is monitored before it is permitted to leave the site if radioactivity is found or suspected. Material packaged for shipment from Livermore is similarly monitored before release to the carrier.

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